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THE QUEEN ANT AS A PSYCHOLOGICAL STUDY

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OBSERVERS of ant behavior have almost invariably fixed their attention on the easily procurable workers, to the all but complete neglect of the males and queens. In the case of the males, this neglect is, perhaps, pardonable, for the behavior of this sex is extremely monotonous. The neglect of the queens, however, as I shall endeavor to show, has not only left untouched a very interesting subject of study, but is responsible for much useless speculation.

Mention of the queen ant unfortunately suggests by association the idea of the queen honey-bee. These two insects are, however, in certain very important respects diametrical opposites. The queen honey-bee is a degenerate creature, unable to nourish herself or her young, to visit the flowers, build or store the comb; while the worker bee, apart from her normal infertility, still retains intact all the true female attributes of the ancestral solitary bees. In ants the very reverse of this is true: the queen is the perfect exemplar and embodiment of the species and has lost none of the primitive female attributes of independence and initiative, which she shares with the female bumble bees, solitary and social wasps. The worker ant, on the contrary, bears all the stigmata of incomplete and retarded development. Although these differences between the queen honey-bee and queen ant and between the respective workers must be apparent to the most superficial observer, yet the familiar conception of the queen honey-bee as little more than an egg-laying machine, so degenerate that she can not exist apart from the workers, has been tacitly expanded to embrace the queen ant. Surely it is time that the reputation of this insect should be viewed in a more favorable light.

Let us follow as briefly as possible the eventful life history of the queen ant. After more protracted larval and pupal stages than those of the worker and male—more protracted in order that she may store up more food and hence more energy in her body—she hatches as a sensitive callow in a colony at the height of its annual development. In other words, she is born into a community teeming with queens, workers and males, and the larvæ and pupæ of these various forms at the season of their greatest activity and growth. From all sides a shower of stimuli must be constantly raining in upon her delicate organization as she tarries for days or even weeks in the dark galleries of the parental

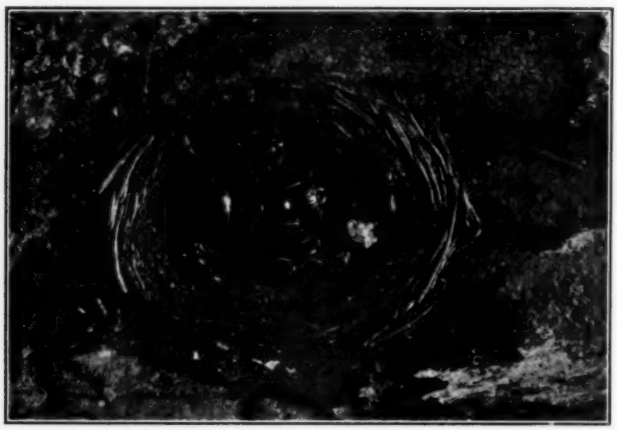

FIG. 1. INCIPIENT COLONY OF CARPENTER ANT (*Camponotus pennsylvanicus*), consisting of the queen, three minim workers and a packet of young larvæ, inhabiting the abandoned pupa case of a beetle (*Rhagium lineatum*) under pine bark. About natural size.

nest, while her color gradually deepens and her integument acquires its mature consistency. During this her prenuptial life, she may assist the workers in carrying about, feeding and cleaning the brood. She eats independently of the food brought into the nest by the foraging workers. She may occasionally join the workers in excavating chambers and galleries. If she belongs to a slave-making species she may even accompany the workers on their cocoon-robbing expeditions. Although she shows that she is able to perform all these actions supposed to be peculiar to the workers, she often does so with a certain desultory incoherency.

When fully mature she becomes impatient for her marriage flight and must often be forcibly detained in the nest by the workers till the propitious hour arrives when the males and females from all the nests in the neighborhood rise high into the air and celebrate their nuptials.

Then the fertilized queen descends to the earth and at once divests herself of her wings, either by pulling them off with her legs and jaws or by rubbing them off against the grass-blades, pebbles or soil. This act of deälation is the signal for important physiological and psychological changes. She is now an isolated being, henceforth restricted to a purely terrestrial existence, and has gone back to the ancestral level of the solitary female Hymenopteron. During her life in the parental nest she stored her body with food in the form of masses of fat and bulky wing-muscles. With this physiological endowment and with an elaborate inherited disposition, ordinarily called instinct, she sets out alone to create a colony out of her own substance. She begins by excavating a small burrow, either in the open soil, under some stone or in rotten wood. She enlarges the blind end of the burrow to form a small chamber and then completely closes the opening to the outside world.

FIG. 2. HEAD OF A RECENTLY FERTILIZED QUEEN OF *Atta sexdens* LONGITUDINALLY BISECTED; *a*, mandible; *b*, labium retracted; *c*, buccal pocket, containing *d*, the pellet of fungus hyphæ carried from the parental nest, *e*, œsophagus, *f*, oral orifice. (After J. Huber.)

The labor of excavating often wears away all her mandibular teeth, rubs the hairs from her body and mars her burnished or sculptured armor, thus producing a number of mutilations, which, though occurring generation after generation in species that nest in hard, stony soil, are, of course, never inherited. In the cloistered seclusion of her chamber the queen now passes days, weeks, or even months, waiting for the eggs to mature in her ovaries. When these eggs have reached their full volume at the expense of her fat-body and degenerating wing-muscles, they are fertilized with a few of the many thousand spermatozoa stored up in her spermatheca and laid. The queen nurses them in a little packet till they hatch as minute larvæ. These she feeds with salivary secretion derived by metabolism from the same source as the eggs, namely, from her fat-body and wing-muscles. The larvæ grow slowly, pupate prematurely and hatch as unusually small but otherwise normal workers. In some species it takes fully ten months to bring such a brood of minim workers to maturity, and during all this time the queen takes no nourishment, but merely draws on her reserve tissues. As soon as the workers mature, they break through the soil and thereby make an entrance to the nest and establish a communication with the outside world. They enlarge the original chamber and continue the excavation in the form of galleries. They go forth in search of food and share it with their exhausted mother, who now exhibits a further and final change in her behavior. She becomes so exceedingly timid

FIG. 3 EGGS AND FUNGUS GARDEN IN CELL OF QUEEN *Atta sexdens* FORTY-EIGHT HOURS AFTER THE NUPTIAL FLIGHT. (After J. Huber.)

FIG. 4. EGGS AND FUNGUS GARDEN IN CELL OF QUEEN *Atta sexdens* SEVENTY-TWO HOURS AFTER THE NUPTIAL FLIGHT. (After J. Huber.)

and sensitive to the light that she hastens to conceal herself on the slightest disturbance to the nest. She becomes utterly indifferent to the young, leaving them entirely to the care of the workers, while she limits her activities to laying eggs and imbibing liquid food from the tongues of her attendants. This copious nourishment soon restores her depleted fat-body, but her disappearing wing-muscles have left her thoracic cavity hollow and filled with gases which cause her to float when placed in water. With this circumscribed activity she lives on, sometimes to an age of fifteen years, as a mere egg-laying machine. The current reputation of the ant queen is derived from such old, abraded, toothless, timorous queens found in well-established colonies. But it is neither chivalrous nor scientific to dwell exclusively on the limitations of these decrepit beldames without calling to mind the charms and self-sacrifices of their younger days.

FIG. 5. SILHOUETTE OF A QUEEN *Atta sexdens* IN THE ACT OF MANURING HER FUNGUS GARDEN. A tuft of fungus mycelium is torn out of the garden, placed against the anus and saturated with a drop of fecal liquid. (From an instantaneous photograph, after J. Huber.)

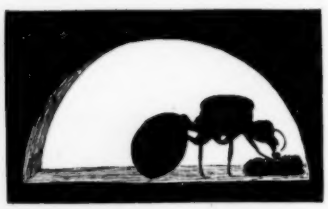

FIG. 6. SILHOUETTE OF A QUEEN *Atta sexdens* REPLACING THE SATURATED TUFT OF MYCELIUM IN THE FUNGUS GARDEN. (From an instantaneous photograph, after J. Huber.)

Now to bring up a family of even very small children without eating anything and entirely on substances abstracted from one's own tissues is no trivial undertaking. Of the many thousands of ant queens annually impelled to enter on this ultra-strenuous life, very few survive to become mothers of colonies. The vast majority, after starting their shallow burrows, perish through excessive drought, moisture or cold, the attacks of parasitic fungi or subterranean insects, or start out with an inade-

quate supply of food-tissue in the first place. Only the very best endowed individuals live to preserve the species from extinction. I know of no better example of natural selection through the survival of the fittest.

It is certain that the colonies of most species are founded in the manner here described. It is certain, moreover, that all this is rendered possible by the nutritive endowment of the queen. As the winged germ of the species she has all the advantages that a yolk-laden has over a comparatively yolkless egg. Now among the 5,000 known species of ants we should expect to find considerable differences in the quantity of nutriment stored up in the young queen. And this is unquestionably the case. In some species the queens are of enormous size, in others they are very small compared with the workers. And since queens of

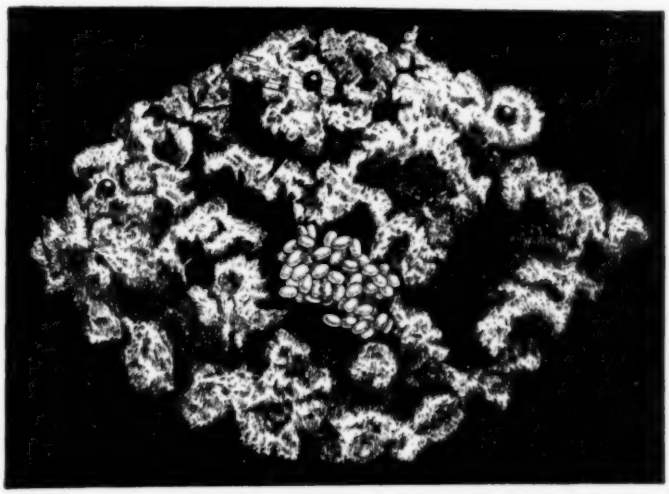

FIG. 7. FUNGUS GARDEN OF *Atta sexdens* FOURTEEN DAYS AFTER THE NUPTIAL FLIGHT. There are about 100 eggs which the queen has placed in a depression in the middle of the garden. Near the periphery there are three drops of the fecal liquid with which the queen manures the garden. (After J. Huber.)

average dimensions are able to start colonies by themselves alone, we should expect that unusually large queens would be able to accomplish even more, and very small queens less. This, too, is borne out by observation.

Unusually large queens are found in the genus *Atta*, a group of American ants that raise fungi for food, and are, so far as known, quite unable to subsist on anything else. The female *Atta* on leaving the parental nest, is so well endowed with food-tissue, that she not only can raise a brood of workers without taking nourishment, but has energy to spare for the cultivation of a kitchen garden. She carries the germ of this garden from the parental nest in the form of a pellet of fungus

hyphæ stowed away in her buccal pocket, spits it out soon after completing her chamber, manures with her excreta the rapidly growing hyphæ and carefully weeds them till her firstling brood of workers hatches. These then bring into the nest the pieces of leaves and the vegetable detritus essential to the maintenance and growth of the garden. The extraordinary habits of one of these fungus-raising ants, *Atta sexdens,* have been recently studied in great detail by Jakob Huber, from whose valuable paper I borrow a number of the accompanying illustrations (Figs. 2 to 7).

Very different is the condition of certain queen ants poorly endowed with food-tissue—especially of some whose bodies are actually smaller than the largest workers of their species. Such queens are quite unable to bring up colonies unaided. They are therefore compelled, after fertilization, to associate themselves with adult workers either of their own or of a closely allied species. In the former case the queens may either remain in the parental nest and omit the nuptial flight, or return to the parental or to some other colony of the same species. In either case they add to the reproductive energy of an already established colony and thus prolong its life. If one of these poorly endowed queens, however, happens to alight from her nuptial journey far from any colony of her own species, she is obliged to associate with alien workers. And in this case, according to the species to which she belongs, one of three courses is open to her.

First, she may secure adoption in a small queenless colony of an allied species. Here she is fed, lays her eggs and the resulting larvæ are reared by the strange workers. Eventually the alien workers die off and leave the queen and her own workers as an independent and sufficiently established colony, capable of rapid and often enormous multiplication. This is temporary social parasitism, first observed by myself in some of our American ants, but since found in some of the European species where I predicted its occurrence.

Second, the poorly endowed queen may establish herself in a colony of another species, but be unable, even after her workers have matured, to survive the death of the host colony, except, perhaps, by migrating to another nest of the same species. This is permanent social parasitism.

Third, the queen may enter a small colony of alien workers, and, when attacked, massacre them, appropriate their larvæ and pupæ, carefully secrete and nurse them till they hatch and thus surround herself with a colony of young and loyal workers that can bring up her brood for her without any drain on her food-tissues. This is the method of colony formation adopted by queens of the slave-making ants, as I have found by a number of experiments during the past summer. These queens thus manifest an instinct, hitherto supposed to be exclusively peculiar to the workers, namely the instinct to rob the larvæ and pupæ of another species and bring them up as auxiliaries, or slaves.

Although the foregoing facts belong to ethology rather than to comparative psychology, it seemed necessary to review them before emphasizing their bearing on certain general questions. The behavior of the queen ant may be said to depend, first, on a relatively fixed inherited predisposition, or instinct; second, on inherited plasticity or adaptability; third, on constantly changing physiological states, and fourth, on stimuli which are partly primary and external and partly secondary, internal or true stimuli. These last are probably identical, as suggested by Jennings and others, with the physiological states, which in turn are evidently to be conceived as metabolic processes. That the queen ant profits by her prenuptial sojourn in the parental nest to learn by experience, tradition and imitation, I have no doubt, but queens hatched in isolation show that this acquisition is insignificant in comparison with the inherited instincts. These appear as elaborate catenary reflexes, of which the reactions to light and contact stimuli may be taken as examples. In her callow stages the queen is negatively phototropic and positively stereotropic, but as the time for her nuptial flight approaches, these reactions are reversed, so that she seeks the light and avoids contact with the walls of the nest. After fertilization she again returns to the prenuptial condition—she shuns the light and tries to bury herself in the soil or under stones. These reactions, first described by Loeb, are as irresistible as they are adaptive. It can be shown, moreover, that these changes in tropisms are accompanied by changes in other instincts. My attention was first directed to the stereotyped character of these reactions in the queen ant by a simple experiment. I found that merely pulling off the wings with the tweezers caused the insect to pass at once from positive phototropism and negative stereotropism to the reverse. This shows that the change is not caused by fecundation, since artificially deälated virgin queens went through the complex catenary reflex of founding a colony with the same precision as fertilized individuals.

These and other observations, which I am unable to give in the space at my disposal, all point to constantly changing metabolic states as the mainspring of the queen ant's behavior. She is, in fact, a veritable chemical laboratory, in which we can see more clearly than in many other animals, a direct relation between behavior and the flux of metabolism.

I hasten over this matter to another general problem. The discovery that the queen ant really possesses, at least *in potentiâ*, all the instincts of the worker, besides others peculiar to herself, puts a different construction on a matter which has long been puzzling theoretical zoologists. It has been taken for granted that worker ants are necessarily sterile and that they possess morphological, physiological and psychological characters not represented in the queens of their

species. On such assumptions it is, of course, impossible to understand how the workers can have come by the obviously adaptive and exquisitely correlated characters, which they are unable to transmit. It will be remembered that neo-Darwinians and neo-Lamarckians, in the persons of Weismann and Herbert Spencer, locked horns over this matter some years ago. Both in this and in many similar discussions, the very premises which both parties accepted are unwarranted. In the first place, it is now known that workers readily become fertile when well fed and that they can and often do produce normal young from unfecundated eggs. Although these young are usually, if not always, males, it is evident that these males, through the eggs which they fertilize, can transmit the characters of their worker mothers to succeeding generations of queens and workers. Thus the congenital, and perhaps even the acquired, characters of the worker are not necessarily lost, but can be gathered up into the germ-plasma of the species. In the second place, most, if not all of the characters of the worker are not qualitatively but only quantitatively different from those of the queen. In other words, the worker does not differ from the queen as a mutant, but as a fluctuating variation, which has been produced by imperfect or irregular feeding during its larval stages. This is true alike of morphological, physiological and psychological characters. Even when the queen fails to manifest the worker instincts, we are not justified in doubting her ability to do so under the proper conditions.

The hitherto unsuspected capacity of the queen ant is beautifully illustrated by another set of facts, which at the same time show the close connection between adaptive behavior and regulation, or regeneration. Under normal conditions the queen, after rearing a brood of workers, no longer takes part in the 'muck and muddle of child-raising' but seems to be as indifferent to the young of her species as some women who have brought up large families. If, however, the firstling brood of workers be removed and the queen isolated, she forthwith begins to bring up another brood, precisely as in the first instance, provided her body still contains sufficient food-tissue. She thus regenerates the lost part of her colony, just as a mutilated earthworm regenerates its lost segments. In the ant the absence of workers acts as a stimulus to restore the colony, just as the absence of segments leads the earthworm to complete its body.

The regulatory activities of the queen ant are, of course, highly adaptive and hence evidence of the variability which is so clearly manifested in the physical structure of these insects. There is no contradiction in the coexistence of such variability with the very stable character of certain instincts like those to which I have called attention, for an organism may be extremely plastic in some of its activities and rigidly conservative in others. It is evident that the remarkable vari-

ability of the female sex in ants—including under this term both the fertile, or queen phase, and the usually sterile, or worker phase—reaches its clearest expression in the extraordinary range of intraspecific polymorphism. In certain species, for example in the African driver ants (*Dorylii*) and American ants of visitation (*Ecitonii*), the structural differences between the workers of the smallest caste and the huge queen of the same species are enormous and represent an amplitude of variability in the female sex unequalled in any other organisms. Male ants, on the contrary, exhibit so little variability that it is often difficult, or even impossible, to distinguish the genera of single specimens of this sex. These facts have an important bearing on the views of authors like Brooks and Geddes and Thomson who assume that male animals are more variable than females, and of those authors who have transplanted this hypothesis to the fields of sociology and anthropology. All of these writers maintain a discreet and significant silence on the subject of the social insects. Equally astonishing, however, is the attitude of the biometricians, who, priding themselves on the accuracy of their methods and repudiating mere observation and speculation, proceed to an elaborate measurement of the wings of honey-bees and ants for the purpose of ascertaining whether males are more variable than females, when a glance at the personnel of a few ant and termite colonies would convince the most skeptical that there can be no such correlation between sex and variability as that assumed by the above-mentioned authors. If it is clear that the males of many of the higher animals are in certain characters more variable than the cospecific females, it is even clearer that the very opposite is true of the social hymenoptera, while in the termites, or white ants, both sexes seem to be alike variable and polymorphic.

THE DEVELOPMENT OF THE GLACIAL HYPOTHESIS IN AMERICA[1]

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GEOLOGY is preeminently a science of observation and deduction. Certain phases of it are, nevertheless, dependent upon advances in other branches. As the science grows this mutual interdependence becomes more and more apparent, and it is perhaps now questionable if further advance, aside from a purely geographic extension of knowledge relative to the distribution of geologic groups, is possible without calling in the aid of physics and chemistry.

Among those phases of geology which have been most independent of the allied sciences, and in which the gradual development of the power to observe correctly and deduce accordingly can incidentally be readily studied, the phenomena of the drift stand prominent. They furnish, moreover, an excellent example of the growth of knowledge through cumulative evidence, since of all phenomena, those relating to the drift have, in America, been perhaps longest under observation. In that which follows, however, it has been my principal aim to trace the gradual progress of the glacial hypothesis in America. Beyond this my references are purely incidental.

Out of numerous observations relating to the phenomena of the drift and, incidentally, to glaciation, those which seem most worthy of consideration at the present time were by Benjamin De Witt and are to be found recorded in the second volume of the 'Transactions' of the Philadelphia Academy for 1793.

De Witt noted the occurrence, along the shores of Lake Superior, of boulders representing a large variety of rocks, sixty-four different types being collected. In the discussion of this occurrence, he remarks:

> Now, it is almost impossible to believe that so great a variety of stones should be naturally formed in one place. . . . They must, therefore, have been conveyed there by some extraordinary means. I am inclined to believe that this may have been effected by some mighty convulsion of nature, such as an earthquake or eruption, and perhaps this vast lake may be considered as one of those great 'fountains of the deep' which were broken up when the earth was deluged with water, thereby producing that confusion and disorder in the composition of its surface which evidently seems to exist.

This, so far as I am aware, is the first attempt to account for the

[1] Adapted from a presidential address delivered December 13, 1905, before the Geological Society of Washington.

wide dispersion of drift boulders over the northern part of the country, although the glacial character of the same was not dreamed of.

The paper, so far as existing literature shows, caused little discussion, and we have next to refer to observations by Dr. Samuel Akerly, who, in 1810, published in Bruce's *American Mineralogical Journal* a geological account of Dutchess County, New York. After referring to the Highlands and the country to the northward, he described the southern part—that upon which the then existing city was built—as composed of an alluvion of sand, stone and rocks. This he looked upon as a recent deposit, 'subsequent to the creation and even the deluge.' The manner in which this material was deposited he described in the following language:

> After the waters of the deluge had retired from this continent, they left a vast chain of lakes, some of which are still confined within their rocky barriers. Others have since broken their bounds and united with the ocean. The Highlands of New York was the southern boundary of a huge collection of water, which was confined on the west by the Shawangunk and Catskill mountains. The hills on the east of the Hudson confined it there. When the hills were cleft and the mountains torn asunder, the water found vent and overflowed the country to the south. The earth, sand, stones and rocks brought down by this torrent were deposited in various places, as on this island, Long Island, Staten Island and the Jerseys. This opinion, he added, is mostly hypothetical, because unsupported by a sufficient number of facts.

A candid acknowledgment upon which the author is to be congratulated!

Whose was the master mind that first conceived of this great barrier which held back the flood of waters, so long made responsible for the drift, I have not been able to ascertain. The theory is, however, given in greatest detail by Dr. Samuel L. Mitchill in his 'Observations on the Geology of North America,' published in 1818.

It was Mitchill's idea that the Great Lakes are the shrunken representatives of great internal seas of salt water, which ultimately broke through their barriers, the remains of which he thought to be still evident. One of them he wrote, seemed to have circumscribed the waters of the original Lake Ontario and to be still traceable as a mountainous ridge beyond the St. Lawrence in upper Canada, passing thence into New York, where it formed the divide between the present lake and the St. Lawrence and continued to the north end of Lake George, apparently crossing the Hudson above Hadley Falls. Thence, he believed it to run toward the eastern sources of the Susquehanna, which it crossed to the north of Harrisburg, and continued in a southeasterly direction until it entered Maryland, passing the Potomac at Harpers Ferry into Virginia, where it became confounded with the Allegheny Mountains.

To appreciate Mitchill's views, then, we have to imagine this now broken and gapped ridge as continuous. A time came, however, when at various points it gave way, the pent-up waters rushing through and

carrying devastation before them like the waters from cloud-burst or bursting reservoirs of to-day, but on a thousandfold larger scale. By this bursting all the country on both the Canadian and Fredonian sides must have been drained and left bare, exposing to view the water-worn pebbles, and the whole exhibition of organic remains there formed. Great masses of primitive rocks from the demolished dam, and vast quantities of sand, mud and gravel were carried down the stream to form the curious admixture of primitive with alluvial materials in the regions below.

A fresh contribution to the subject was rendered this same year, in the publication of Amos Eaton's 'Index to the Geology of the Northern States.' Eaton's views were in part, at least, a reflection of those of Werner. We have to do here, however, only with that portion of the work relating to the so-called alluvial class of rocks. In discussing this and attempting to account for the great masses of granite and syenite which he found scattered throughout the Connecticut River region, he wrote:

> What force can have brought these masses from the western hills, across a deep valley seven hundred feet lower than their present situation? Are we not compelled to say that this valley was once filled up so as to make a gradual descent from the Chesterfield range of granite, syenite, etc., to the top of Mount Tom? Then it would be easy to conceive of their being rolled down to the top of the greenstone where we now find them.

It was not easy in all cases for the geologists of these early days to distinguish between the younger and earlier drift, or between the material which we now consider as glacial drift and the loosely consolidated alluvial deposits of the Tertiary period. This seems particularly true of Dr. H. H. Hayden, a Baltimore dentist and one-time architect, who in 1820 published a volume of geological essays in which he dwelt very fully upon the lowlands, or the area at present comprised within the so-called Coastal Plain. After referring to the geographical limits of this plain and combating the opinions of previous observers, he elaborated his own theories somewhat as follows:

> Viewing the subject in all its bearings, there is no circumstance that affords so strong evidence of the cause of the formation of this plane as that of its having been deposited by a general current which, at some unknown period, flowed impetuously across the whole continent of North America in a northeast and southwest direction, its course being dependent upon that of the general current of the Atlantic Ocean, the waters of which were assumed to have risen to such a height that it overran its limits and spread desolation on its ancient shores.

In seeking the cause of this general current Hayden referred first to the seventh chapter of Genesis:

> For yet seven days and I will cause it to rain upon the earth forty days and forty nights, and every living substance that I have made will I destroy from off the face of the earth.

He then proceeded to show the inadequacy of rainfall alone, since the water being thus equally distributed over the ocean and the

land, there could be no tendency toward a current. Some other cause must be sought, and, fortunately, his imagination proved equal to the task.

Accepting as probable the suggestion of one whom he designates as 'a writer of no common celebrity,' to the effect that the cause of the general deluge was a melting of the ice at the two poles of the earth, he proceeded to explain in his own way the details of the catastrophe, though acknowledging that no positive testimony could possibly be adduced to substantiate the fact.

Having admitted the possibility of the earth's changing its position, so that the sun would pass immediately over the two poles on an unknown meridian, he showed that there would then result a rapid dissolution of the existing ice-caps such as would yield an ample supply of material, it being only necessary to give it direction. Considering as essential to the problem only the northern hemisphere, he remarks that, from this polar cap there were but two outlets, the one into the Pacific through the narrow Bering Straits, and the other through the wider channel between Greenland and Lapland into the Atlantic. Hence, when the melting ensued, by far the larger volume of water passed into the latter ocean. No sooner was this operation established and this accession of strength and power thrown into the Atlantic Ocean in particular, than its tide began to rise above its common limits, accompanied by a consequent current, both constantly increasing, the one in height and the other in rapidity.

At the commencement of this frightful drama the current, it is highly probable, was divided by the craggy heights of Spitzbergen and a part thrown into the White Sea, while the other was thrown back upon the eastern and southern coast of Greenland and thence in a southwestern direction until it struck the southern coast of Labrador, along which it swept, through the straits of Belle Isle, across Newfoundland, Nova Scotia, and along the Atlantic coast into the Gulf of Mexico. In a short space of time the southern and eastern coast of Labrador was desolated. The soil was hurled adrift and carried across the country into the Gulf of St. Lawrence and across a part of New England into the sea or general current of the ocean.

Continuing to rise, the waters spread across Davis Straits and rolled their tumultuous surges into Hudsons Bay, embracing the whole coast of Labrador, while the current of the St. Lawrence was forced back and upward to its parent source.

At length the floods of the pole, forming a junction with Baffins Bay and the Arctic Sea, defying all bounds, overran their ancient limits and hurled their united forces in dread confusion across the bleak regions of the north to consummate the awful scene. Thus, lakes and seas uniting, formed one common ocean which was propelled with inconceivable rapidity across the continent between the chains of mountains, into the Gulf of Mexico, and probably over the unpeopled wilds of South America into the southern ocean. Fulfilled

in this way were the awful denunciations of an offended God, by the sure extermination of every beast of the field and every creeping thing that crawled upon the face of the earth.

To these causes he believed to be due, not merely the deposits of the coastal plain, but, as well, the barrenness of Labrador and the northeastern portion of the continent, and the general phenomena of the glacial drift, the boulders of the latter being conceived as transported by floating ice.

These essays, it may be noted, were favorably reviewed by Silliman in the third volume of his Journal, even the idea of the fusion of the polar ice-cap being allowed to pass with no more serious criticism than that the flood of waters might have been produced through the expulsion of the same from cavities in the earth. J. E. DeKay, however, writing some years later, ventured to take exception to the views regarding drift boulders, wisely suggesting that, since the speculative part of geology is but a series of hypotheses, we should in every case admit that which explains the phenomena in the simplest possible manner. To his mind the simplest manner of accounting for these boulders was to suppose that such had been, as igneous material, extruded through the superincumbent strata, forming peaks which have since been destroyed through some convulsion of nature or through the resistless tooth of time, the boulders thus being fragments which had escaped destruction, though their place of extrusion had become completely obscured.

The observations thus far recorded display a lack of close attention to details and, in some cases, indicate a decided leaning towards cosmogony. Those to which I now call attention were of quite different type and show their author to have been a man of more than ordinary discernment.

While superintending excavations preliminary to the erection of a cotton mill at Vernon, Conn., Mr. Peter Dobson observed the boulders in what we now call the ground moraine or till, and in a letter dated November 21, 1825, described them as worn smooth on their under side as if done by having been dragged over rocks and gravelly earth in one steady position. They also showed scratches and furrows on the abraded parts. These appearances he could account for only by assuming that the blocks had been worn by being carried in ice over rocks and earth under water.

These observations seem to have attracted no attention at the time, and even Dr. Edward Hitchcock thirteen years later attached no serious importance to them, although his attention was called to the matter by another letter from Dobson, this time addressed to himself. In this last letter, written in 1838, Dobson described the boulders as having first been rounded by attrition and then worn flat on one side

by a motion that kept them in one relative position, as a plane slides over a board in the act of planing. Some of them were described as worn and scratched so plainly that there was no difficulty in pointing out which side was foremost in the act of wearing, a projecting bit of quartz or feldspar protecting the softer material behind it. In this letter he again announced his inability to account for the appearances except on the supposition that they had been enveloped in ice and moved forward over the sea bottom by currents of water. The drifting icebergs of the Labrador coast he thought might well illustrate the conditions of their production.

Perhaps it may have been because Dobson was a cotton manufacturer and not a member of one of the learned professions, or there may have been other reasons, but Hitchcock allowed the observations to pass unnoticed until 1842, when the subject was brought up by Sir Roderick Murchison in his anniversary address before the Geological Society of London.

> I take leave of the glacial theory in congratulating American science in having possessed the original author of the best glacial theory, though his name has escaped notice, and in recommending to you the terse argument of Peter Dobson, a previous acquaintance with which might have saved volumes of disputation on both sides of the Atlantic.

Supported by this somewhat enthusisastic endorsement, Hitchcock then gave the letter to the public through the *American Journal of Science,* at the same time remarking that he had himself derived his ideas concerning the joint action of ice and water from the writings of Sir James Hall.

With this much in the way of anticipation, I will turn back to 1825 once more and refer to the writings of William Keating, mineralogist, who accompanied Major Long's expedition to the sources of the St. Peters River. This observer noted that the entire region of the present headwaters of the Winnipeek River had been at a comparatively recent period an immense lake, interspersed with innumerable barren rock islands, which had been drained by the bursting of the barriers which tided back the waters. This was plainly a recognition of the now extinct glacial Lake Agassiz.

Although the cosmogonist was fast drifting into the obscurity of the past, there were, nevertheless, occasional writers who preferred tc ignore facts of observation or the efficiency of simple causes, and to seek for more difficult or more mystical methods of accounting for phenomena than those afforded by the observation of processes now in action. Thus, Benjamin Tappan, in discussing in 1828 the boulders of primitive and transition rocks found in Ohio, objected to the commonly accepted idea that such were necessarily foreign to the locality and brought by currents of water or floating ice. He frankly acknowl-

edged, however, his own inability to account for their presence, but ingenuously claimed that

> Ignorance is preferable to error. It may, therefore, be asked why may not these rocks have been created where they are now found, or, again, why may they not have been thrown up by earthquakes or volcanoes?

Groping though this writer may have been, it is questionable if his ignorance was not preferable to the kind of knowledge manifested by a writer in the *American Journal of Science* two years previously, who had accounted for the drift on the supposition that the earth's revolution, amounting to 1,500 feet a second, was suddenly checked. This, he thought, would result in the whole mass of the surface water rushing forward with inconceivable velocity until overcome by opposing obstacles or exhausted by continual friction and the counterbalancing power of gravitation. The Pacific Ocean would thus rush over the Andes and the Alleghenies into the Atlantic, which would, in the meantime, be sweeping over Europe, Asia and Africa.

> A few hours would cover the entire surface of the earth, excepting, perhaps, the vicinity of the poles, with one rushing torrent in which the fragments of disintegrated rocks, earth, and sand would be carried along with the wreck of animal and vegetable life in one all but liquid mass.

The first geological survey of an entire state carried through at public expense was that of Massachusetts, authorized by legislative enactment in 1830. Dr. Edward Hitchcock, then professor of chemistry and natural history in Amherst College, was selected to carry out the work. The report presented early in 1832 was, therefore, a document of unusual importance and, to a certain extent, epoch-making. Much that is of interest is to be found within its pages, but we must limit ourselves to that relating purely to the distribution of the numerous erratics for which the state is noted.

It is but natural that this drift should have been attributed to the Noachian deluge, when one considers that Hitchcock's training was that of a clergyman. Speaking of that about Cape Ann, he wrote:

> It can not fail to impress every reasoning mind with the conviction that a deluge of tremendous power must have swept over this cape. Nothing but a substratum of syenite could have stood before its devastating energy.

This observation is of importance, since here, for the first time, Hitchcock put himself on record in a line of investigation in which he became more widely known than in any other, with the possible exception of that relating to the fossil footprints of the Connecticut Valley.

In 1836 there was established a state geological survey of Maine, with C. T. Jackson, of Boston, at its head. Jackson's views on the glacial deposits, as expressed in the annual reports, were perhaps not more crude than those of the average geologist of his day. The 'horseback' (ridges of glacial gravel) were regarded by him as of diluvial

material, transported by a mighty current of water which, he supposed, rushed over the land during the last grand deluge, accounts of which had been handed down by tradition and preserved in the archives of all people. "Although," he says, "it is commonly supposed that the deluge was intended solely for the punishment of the corrupt antediluvians, it is not improbable that the descendants of Noah reap many advantages from its influence, since the various soils underwent modifications and admixtures which render them better adapted for the wants of man. May not the hand of benevolence be seen working even amid the waters of the deluge?" he asks. It is, perhaps, doubtful if the hard-fisted occupants of many of Maine's rocky farms would be disposed to take so cheerful a view of the matter.

Substantially the same views were advanced by Jackson in his report on the geology of Rhode Island, which appeared in 1839.

> There can not remain a doubt but that a violent current of water has rushed over the surface of the state since the elevation and consolidation of all the rocks and subsequent to the deposition of the Tertiary clay, and that this current came from the north. . . . Upon the surface of solid ledges, wherever they have been recently uncovered of their soil, scratches are seen running north and south, and the hard rocks are more or less polished by the currents of water which at the diluvial epoch coursed over their surfaces, carrying along the pebbles and sand which effected this abrasion, leaving striæ, all of which run north and south, deviating a few degrees occasionally with the changes of direction given to the current by the obstacles in its way.

He did not accept the theory of drifting icebergs; 'nor,' he wrote, 'can we allow that any glaciers could have produced them by their loads of sliding rocks, for in that case they should have radiated from the mountains instead of following a uniform course along hillsides and through valleys.'

Although primarily a paleontologist, Timothy Conrad was sometimes drawn out of his chosen field by phenomena too obvious to be overlooked and concerning the nature of which little was actually known by the best authorities. The occurrence of enormous boulders in the drift, resting often upon unconsolidated sand and gravel, fell within this category. That such could not have been brought into their present position through floods was to him obvious, neither could they have been floated by ice floes from the north during a period of terrestrial depression. He assumed, rather, that the country previous to what is now known as the glacial epoch was covered with enormous lakes, and that a change in climate ensued, causing them to become frozen and converted into immense glaciers. At the same time elevations and depressions of the earth's surface were in progress, giving various degrees of inclination to the frozen surfaces of the lakes, down which boulders, sand and gravel would be impelled to great distances from the points of their origin. The impelling force, he thought, in some cases might be gravity alone, but during the close of the epoch,

when the temperature had risen, vast landslides—avalanches of mud filled with detritus—would be propelled for many miles over these frozen lakes, and when the ice disappeared, the same would be deposited in the form of a promiscuous aggregate of sand, gravels, pebbles and boulders.

In 1840 an immense stride in the study of drift deposits was made through the publication of Louis Agassiz's 'Etude sur les Glaciers,' a work comprising the results of his own study and observation combined with those of Jan de Charpentier, E. T. Venetz and F. G. Hugi. The work was published in both French and German, and brought to a focus, as it were, the scattered rays by which the obscure path of the glacial geologist had been heretofore illuminated. It was Agassiz's idea, as is well known, that at a period geologically very recent, the entire hemisphere north of the thirty-fifth and thirty-sixth parallels had been covered by a sheet of ice possessing all the characteristics of existing glaciers in the Swiss Alps. Through this agency he would account for the loose beds of sand and gravel, the boulder clays, erratics, and all the numerous phenomena within the region described, which had been heretofore variously ascribed to the Noachian deluge, the bursting of dams, the sudden melting of a polar ice-cap, or even to cometary collisions with the earth. These ideas were favorably received by the majority of workers, though there was, naturally, a highly commendable feeling of caution against their too hasty acceptation. As a reviewer in the *American Journal of Science* expressed it:

> These very original and ingenious speculations of Professor Agassiz must be held for the present to be under trial. They have been deduced from the limited number of facts observed by himself and others and skillfully generalized, but they can not be considered as fully established until they have been brought to the test of observation in different parts of the world and under a great variety of circumstances.

The effect of this publication, however, soon made itself apparent in the current literature. Thus, in 1843 Professor Charles Dewey, writing on the striæ and furrows on the polished rocks of western New York, argued that, while the boulders of the drift indicated that a mighty current had swept from north to south, the polishing and grooving might be due to glaciers. 'Glaciers or icebergs and the strong currents of water—a union of two powerful causes—probably offer the least objectionable solution of those wonderful changes,' he wrote. Though thus disposed to accept in part Agassiz's conclusions, Dewey yet failed to realize their full possibilities.

He could not conceive how it was possible for a glacier to transport sandstone boulders from the shore of Lake Ontario to the higher level of the hills to the southward. Boulders of graywacke removed from the hills in the adjoining part of the state of New York and scattered throughout the Housatonic Valley furnished a like difficulty, since

between the place of origin and that of deposit lay the Taconic range of mountains. "If," he wrote, "the boulders were once lodged on the glacier, the ice and boulders must have been transported by a flood of waters over the Taconic Mountains."

In 1842 Dr. Edward Hitchcock, already referred to in connection with Dobson's work, read an important paper before the Boston meeting of the American Association of Geologists on the phenomena we are now discussing, which is particularly interesting as showing the gradual evolution of the present theory from that of the Noachian deluge idea, advocated by the earlier workers. His views had evidently been modified by the publication of Agassiz's work and by the writings of Buckland, Murchison, Dobson and others, and he expressed at the outset the conviction that nearly all geologists would agree in the principle that the phenomena of drift were the result of joint and alternate action of ice and water. To express this joint and alternate action he made use of the term aqueo-glacial.

In this paper Hitchcock devoted some fifty pages to a description and discussion of observed phenomena, after which he proceeded to discuss the theories of the various European authorities and state his agreement or objections to the same. He objected to the theory of Lyell, to the effect that the results observed by him in North America were produced by floating icebergs derived from glaciers formed on mountains as the land gradually emerged from the ocean, because, first, it failed to account for the lower temperature which was necessary; second, because there was no evidence that the glaciers descended from the mountains; and third, because the deposits of vegetable matter derived from land plants showed that the continent must have been above sea-level long before the drift period.

The theory of De la Beche, which supposed the contents of the northern ocean to have been precipitated over the countries further south by the elevation of the polar regions, Hitchcock regarded as possibly applicable to the low countries of Europe, but not to New England, since it would require a rise of the ocean amounting to some 6,000 feet, and he could find no facts to justify such an assumption, although recognizing the fact that the aqueo-glacial agency had operated well over the summits of the White Mountains.

To Agassiz's theory, which supposed an immense accumulation of ice and snow around the poles during the glacial period and a consequent sending out of enormous glaciers in a southern direction, followed by enormous floods of water and transportation of icebergs on return of a warmer period, he likewise took exception, since he was unable to conceive how such effects could be brought about. Nor, indeed, could he understand how such causes could operate when the land was rising from the ocean and the water consequently retreating,

as it must have been to account for the various observed phenomena—such phenomena as would necessitate the occurrence of water loaded with ice and detritus floating for centuries at least over a large part of the earth's surface.

His paper showed a very clear insight into what had taken place, but an inability, with the information at that time available, to account for it in a satisfactory manner. Thus, in describing the striæ found by himself on the top of hills and mountains like Monadnoc, he wrote:

> Could immense icebergs have been stranded on the northern slope of the hills and afterwards, by the force of currents, have been driven over the summits; or would it be necessary to suppose that, after the stranding, the water must have risen so as to lift the iceberg; or would a vast sheet of ice lying upon the earth's surface, by mere expansion, without the presence of water, have been able to produce the smoothing and furrowing in question?

After considering the phenomena and weighing all the theories advanced from time to time by the authorities quoted, he summed up the matter in the following words:

> Is it not possible that the phenomena of the drift may have resulted from all the causes advanced in the theories under consideration? I feel . . . that the proximate cause of the phenomena of drift has at last been determined, namely, the joint action of water and ice.

In 1836 there was organized a state geological survey of New York, which was placed in charge of W. W. Mather, Ebenezer Emmons, Lardner Vanuxem and James Hall—men whose names have since become too thoroughly identified with American geology to ever become eradicated. Naturally drift phenomena attracted the attention of these workers, and each expressed opinions, some of which may be referred to in detail.

Seventy-five pages of Mather's report, as published in 1843, were given up to descriptions and discussions of drift phenomena. He concluded that the transport of the material and the production of scratched surfaces were contemporaneous, the drift itself being transported in part by currents and in part by ice itself drifted by the currents. The period of the drift and that of the quaternary deposits were separated by a partial submergence of the land, and, further, the periods of the drift were periods when the currents were stronger than at the present time. He conceived this to be due to a collapse of the crust of the globe upon its nucleus, causing an acceleration of the velocity of rotation, and this causing, in its turn, a disturbance of the form of equilibrium of the spheroid of rotation which had been compensated by the flow from the polar regions and an accelerated flow to the equatorial regions. This sudden acceleration of the ocean currents he felt would be sufficient to cause the transportation of vast quantities of detritus-laden ice from the polar regions southward. The large amount of drift scattered over the central and northern Mississippi region he ascribed incidentally to ice-laden currents from

Hudsons Bay and the polar seas, which, floating over the northern part of the United States, would be met by the warm waters of the Gulf Stream, causing them to deposit their loads. The warm current flowing northward would be superimposed on the cold current, the latter continuing southward beneath it, transporting the finer materials, such as now occupy the lower Mississippi Valley.

Emmons likewise believed the agent of drift transportation to be water and ice. The boulders he thought to be the work of icebergs, but the striations and polishing he felt could not be due to this agency, since the bottom of the ocean is not bare rock, but covered by debris, and, moreover, icebergs would not move in straight lines, a point which some more recent writers have quite overlooked. The bergs might act as agents of transportation, he argued, but not of erosion. According to his ideas the drift-covered region was, during the drift period, depressed, the country low and connected at the north with an extensive region giving rise to large rivers which flowed in succession over different parts of the region lying between Champlain and the St. Lawrence. These rivers united with the Atlantic on the south through the Champlain, Hudson and Mohawk valleys. They bore along ice loaded with sand, pebbles, etc., which scratched and grooved the surface of rocks over which they flowed, and were the agents also of perforating the rocks in the form of pot holes.

Hall's ideas were somewhat hazy. That he did not accept Agassiz's doctrine of a vast ice sheet is very evident. Thus, he wrote that

> Blocks of granite, either enclosed in ice or moved by other means, have been the principal agents effecting the diluvial phenomena; that they have scored and grooved the rocks in their passage and, breaking up the strata and mingling themselves with the mass, have been drifted onward carrying everything before them in one general mêlée. That such may have been the case in some instances or in limited localities, can not be denied; but that it ever has been over any great extent of country will scarcely admit of proof.

Hall was at this time evidently a catastrophist and regarded the drift soils, terraces, and the deep valleys and water courses as due to the violent action of water which may have been caused in part by a sudden submergence and the rapid passage of a wave over its surface. His views, indeed, were in many respects little, if any, in advance of those held by Mitchill twenty-five years earlier. Like Mitchill, he conceived of an inland sea bounded and held back by the Canadian highlands on the north, the New England range on the east, and highlands of New York and the Alleghenies on the south, and the Rocky Mountains on the west. These presented barriers of from one thousand to twelve hundred feet above the level of the ocean until broken through by the St. Lawrence, the Susquehanna, the Hudson, partially by the Mohawk at Little Falls, and perhaps also by the Connecticut.

But to whatever cause we attribute the phenomena of the superficial detritus of the fourth district, the whole surface has been permanently covered by water, for it seems impossible that partial inundations could have produced the uniform character and disposition of the materials which we find spread over the surface.

Under the caption 'Description of a Singular Case of Dispersion of Blocks of Stone Connected with the Drift, in Berkshire County, Mass.,' Dr. Edward Hitchcock came forward in the *American Journal of Science* for 1845 with a description of that remarkable train of boulders extending from Fry's Hill in the Canaan Mountains of New York, southeasterly into Massachusetts for a distance of some fifteen or twenty miles, which have since become more generally known as the Richmond Boulder Train. The lithological nature of the boulders was such that they could be traced to a common source and were described as forming three somewhat meandering trains, extending from Fry's Hill, through the adjoining valley, and upwards over an elevation of eight hundred feet at the state line, across the Richmond valley, over Lenox Mountain, six hundred feet in height, to and over Beartown Mountain, one thousand feet in height. Naturally, so striking a phenomenon excited investigation, and, naturally, too, Dr. Hitchcock, in the then existing condition of knowledge, found difficulty in accounting for the same. He recognized the similarity of the trains to the lateral moraines described by Agassiz, but he could not conceive of a glacier traveling directly across the intervening ridges, even were there mountains in the vicinity of sufficient altitude to give rise to the same. Neither did the consideration of river drift or floating ice afford him a satisfactory conclusion:

In short, I find so many difficulties on any supposition which I may make that I prefer to leave the case unexplained until more analogous facts have been observed.

Unsatisfactory and apparently unimportant as this paper may at first thought seem, it is questionable if the contribution were not worthy of greater commendation than the one put forward three years later by the Rogers brothers, to which I now refer.

According to the descriptions given, the trains start, each from its particular depression in the summit of a high ridge in Canaan, N. Y. Taking a direction south 35° east, they cross the higher ridges and their intervening valleys, the longer for a distance of twenty miles and the shorter for ten miles. The individual trains are none of them more than three hundred or four hundred feet in breadth and not over half a mile asunder. The transported blocks of all sizes up to twenty feet in diameter, sharply angular, free from scratches, and all of the same lithological nature, identical with that of the ridge whence they start. That such a dispersion of boulders from a single point should have taken place regardless of contours is certainly enough to excite the interest of any one. It is the means invoked by the two workers which have excited our wonder, however.

After exhibiting to their own satisfaction the inadequacy of either the iceberg or the glacial hypothesis to account for their production, the authors attempted to show how all the phenomena might be explained by the theory of a sudden discharge of a portion of the Arctic Ocean southward across the land. They discussed the important functions of the 'wave of translation,' showing its surpassing velocity and great propulsive power, and traced the influence of vehement earthquakes near the pole in dislodging the northern waters and ice and maintaining in the rushing flood these vast and potent waves. They then suggested that, at a certain stage of the inundation, the ice, previously floating free, might impinge with irresistible violence against the tops of submerged hills, and that the Canaan Mountain stood precisely in the position to take the brunt of the ice-driving flood as it swept down the long, high slope of the distant Adirondacks and across the low, broad valley of the Hudson.

They then proceeded to show that, at the instant when some enormous ice island struck the crest of the mountain and scooped the trench which we there behold, a great vortex was produced by the obstruction thus suddenly thrown in the path of the current, which, endowed with an excessive gyratory or spiral velocity, was capable of sustaining and carrying forward the greater part of the fragments. As in the instance of the waterspout and the whirlwind, the whirlpool would gather into the rotating column the projected blocks and strew them in a narrow path in the line along which its pendent apex would drag the ground.

Truly there were catastrophists in those days!

Agassiz, it will be remembered, came to America in 1846, and in 1847 was appointed to the professorship of geology and zoology in Harvard. Naturally, an attempt was made to apply his views on glaciation to the phenomena of the drift in America. In the summer of 1848, in company with Jules Marcou and a party of students, he undertook the exploration of the Lake Superior region, the results of which were published in 1850. The views set forth relating to the glacial phenomena of the region are of paramount interest.

He argued that the drift of all northeast America and northwest Europe was contemporaneous and due to a general ice sheet. Through a repetition of many of his former arguments, he showed that a current of water sufficiently powerful to transport the large blocks found would have swept practically over the entire globe and not have stopped abruptly, as did the drift, after reaching latitude 39° north. This limit of distribution of the boulders to the northern latitudes also indicated to his mind that the matter of climate was an important factor. Water-transported material, he argued, would not cause straight furrows and scratches, and the theory that such might be due to drifting icebergs was rejected on the ground that existing bergs were insuffi-

cient, and to produce such as were would necessitate a period of cold sufficient for his hypothetical polar ice-cap. He pointed out that the northern erratics were rounded and widespread; that the highest hills were scratched and polished to their summits, while to the south the mountain tops had protruded above the ice-sheet and supplied the glaciers with their load of angular boulders. He also called attention to the absence of marine or fresh-water shells from the ground moraine deposits, showing that it was not subaqueous.

Referring to the stratified deposits overlying the drift, he wrote:

> The various heights at which these stratified deposits occur above the level of the sea show plainly that since their accumulation the mainland has been lifted above the ocean at different rates in different parts of the country; further, it must be at once obvious that the various kinds of loose material all over the northern hemisphere have been accumulated, not only under different conditions, but during long-continued subsequent periods. To the first, or ice, period belong all phenomena connected with the transportation of erratic boulders, polishing, scratching, etc., during which the land stood at a higher level. To the second period belongs the stratified drift such as indicates a depression of the continent.

In 1856 Dr. Edward Hitchcock came once more to the front, through the medium of the 'Smithsonian Contributions to Knowledge,' with a paper of some 150 royal quarto pages and 12 plates, in which he considered the changes which had taken place in the earth's surface since the close of the Tertiary period. The products of these changes he classed as, first, drift unmodified, and second, drift modified, including under the latter such deposits as ancient and modern beaches, submarine ridges, sea bottoms, osars, dunes, terraces, deltas and moraines. The drift proper he regarded, as before, as a product of several agencies, including icebergs, glaciers, land slips and waves of translation, which, though more active in the past than now, are still in operation.

To account for the drift accumulations at various altitudes he conceived that the water must have stood some 2,500 feet above its present level and, further, that all the northern part of the continent—at least all east of the Mississippi—had been covered by the ocean since the drift period.

As to the origin of the material of the irregular coarse deposit beneath the modified beaches and terraces (ground moraine), he agreed essentially with Naumann in supposing that, first, the eroding materials must have been comminuted stone; second, they must have been borne along under heavy pressure; third, the moving force must have operated slowly and with prodigious energy; and fourth, moving in a nearly uniform direction, though liable to local divergence; fifth, the vehicle of the eroded material could not have been water alone; but, sixth, a firm and heavy mass, somewhat plastic. The exact period of operation of the drift agency he naturally found difficulty in deter-

mining, and felt that, while the greater part of the work was accomplished before the continent had emerged very considerably from the waters, nevertheless, the work of erosion went on for some time after emergence began.

It was in this connection that was made the first suggestion, so far as I am aware, of a possible recurrence of glacial periods, as fully elaborated later by Chamberlin. Referring to the occurrence of two series of striæ, the direction of which did not coincide, and the possible existence of still a third series, he wrote: 'Perhaps there were two periods of glaciers, one before, and one subsequent to the drift.'

The facts concerning the dispersion of boulders Hitchcock thought could be more satisfactorily explained by icebergs than glaciers, since the transportation and scattering continued until after the time when a large part of the beaches and terraces were formed. Glaciers, he thought, would have plowed tracks through stratified deposits. Icebergs such as now traverse the Atlantic might carry boulders over the beaches and terraces and drop them from time to time, forming thus the intermixture of coarse angular blocks and beach and terrace material, as we now find it.

The supposition that a glacier once existed on this continent wide enough to reach from Newfoundland to the Rocky Mountains is the grand difficulty in the way of the glacial theory.

The writings of the Rogers brothers are singularly lacking in more than casual references to the drift, though, in one case, at least (that relating to the Richmond boulder train) they advanced some theories which were extraordinary, to say the least. In the, for its time, magnificent publication of the first geological survey report of Pennsylvania one would naturally look for an extension of the views of Professor H. D. Rogers, but such, nevertheless, are not found. The fact that he considered, if not fully comprehended, Agassiz's views is shown only by a brief paragraph in which he described and figured drift striæ seen on an exposed surface of umbral sandstone on the south side of the Wyoming Valley. These he described as 'pointing up the slope toward the southwest, as if produced by fragmentary debris violently propelled against the slope of the mountain wall of the valley from the south.' The presence of such ascending striæ, both here and elsewhere, effectually refuted, according to his conception of it, the glacial theory of their origin.

Like Hitchcock, he failed to conceive of other than local mountain glaciers of the Swiss type, and he gave the following, even then antiquated, matter for a general discussion of the distribution of the drift and the various phenomena accompanying it. Of the earlier drift, it should be noted, he offered no explanation whatever, other than that implied in a reference to a period of repose 'which separated the convulsed epochs of the earlier general and later local drift.'

A ready explanation of the origin of this newest Pleistocene deposit (*i. e.*, that of the Hudson and Lake Champlain districts) suggests itself when we consider the nature and energy of the crustal movements which lifted the Laurentian clays and sands to a height, in one locality at least, of not less than five hundred feet, and which drained wide tracts of the upper Laurentian Lakes.

The mere agitation or pulsating movement of the crust, if accompanied by any permanent uplift of the land, would suffice, we would think, by lashing the waters of the tidal estuaries in one quarter and the lakes in the other, to strew a portion of the older drift bordering all those basins in wide dispersion upon the top of the more tranquil sediments. But if such a pulsation of the crust were accompanied by successively paroxysmal liftings of wide tracts of the land, then the inundation would take the form of stupendous currents, the strewing power of which would be adequate to any amount of superficial transportation, even to the remote transportation of the larger erratics.

In 1861 the Natural History Survey of Maine was inaugurated and C. H. Hitchcock was placed in charge of that portion relating to geology. Of his work, only that relating to glaciers concerns us here. He noted that the fossiliferous marine clays which were regarded as of the same age as similar deposits along the St. Lawrence and Champlain valleys and referable to the terrace period, sometimes underlay a coarse deposit referable to the modified drift. Without committing himself definitely on this point, he suggested the possibility, therefore, of a recurrence of the drift agencies, that is, a period of second drift, as had the elder Hitchcock fifteen years earlier.

The drift period itself, according to Hitchcock's view as here expressed, was inaugurated by a depression of this portion of the continent amounting to at least 5,000 feet below that of to-day, and it was during this period of depression and reelevation that the drift deposits were formed through the joint agency of icebergs and glaciers.

In 1862 J. S. Newberry expressed his views on glaciers in an article on the 'Surface Geology of the Basins of the Great Lakes.' After reviewing the surface conditions as he saw them, he came to the conclusion that, at a period corresponding in climate, if not in time, with the glacial epoch of the old world, the lake region, in common with all the northern portion of the American continent, was raised several thousand feet above the level of the sea. This was to him the glacial period, during which the surface of the country was planed down and the deep fiords along the Atlantic coast formed. This was followed by a period of depression, when all the basin of the Great Lakes was flooded with fresh water, forming a vast inland sea in which the laminated blue clays (the oldest drift deposits) were precipitated.

Subsequent to this deposit of blue clay an immense quantity of gravel and boulders was transported from the region north of the Great Lakes and scattered over a wide area south of them. This he regarded as due to floating ice and icebergs.

It would seem that, if one were looking for original observations on drift phenomena, he might turn with safety to the writings of the

Canadian geologists. Singularly enough, the views are, if anything, less original than those of the workers on this side of the line. The establishment of a geological survey of Canada under Logan led to the publication of the now well-known volume of 1863. The views here expressed may be accepted as a summary of the knowledge relating to the glaciation on Canadian territory, as it then existed.

Concerning the region of the lake basins of western Canada, Logan wrote:

> These great lake basins are depressions, not of geological structure, but of denudation, and the grooves of the surface rocks which descend under their waters appear to point to glacial action as one of the great causes which have produced these depressions. This hypothesis points to a glacial period when the whole region was elevated far above its present level and when the Laurentides, the Adirondacks, and the Green Mountains were lofty Alpine ranges covered with perpetual snow from which great frozen rivers or glaciers extended over the plains below, producing by their movements the glacial drift and scooping out the river valleys and the basins of the great lakes.

In his address before the Natural History Society of Montreal in 1864 J. W. Dawson took occasion to combat vigorously these ideas of Logan, and this on the ground that 'it requires a series of suppositions unlikely in themselves and not warranted by facts'; that it seems physically impossible for a sheet of ice to move over an even surface striating it in uniform directions over vast areas; that glaciers could not have transported the large boulders and left them in the positions found, having no source of supply; that the peat deposits, fossils, etc., show that the sea at that period had much the same temperature as the present arctic currents, and that the land was not covered by ice.

In describing the course of the rock striæ he announced that he had no hesitation in asserting that the force which produced those having a westerly direction was from the ocean into the interior against the slope of the St. Lawrence Valley, and as he could not conceive of a glacier moving from the Atlantic up into the interior, he considered this as at once disposing of the glacial theory. He conceived, rather, that a subsidence took place sufficient to convert all the plains of Canada, New York, and New England into seas. This, he felt, would determine the direction of the Arctic current which moved up the slope. He would account for the excavations of the basins of the Great Lakes by supposing the land so far submerged that an Arctic current from the northeast would pour over the Laurentian rocks on the northern side of Lake Superior and Lake Huron, cutting out the softer strata and at the same time transporting the debris in the form of drift to the southwest. Glaciers were not wholly dispensed with, but limited to regions of mountainous elevation.

J. S. Newberry, while director of the geological survey of Ohio (1869–78) had frequent occasion to discuss glacial phenomena, and a

review of his opinions may be given in some detail. Beginning with the later Tertiary times, he believed the following sequence of events to have been established.

(*a*) During Miocene and Pliocene epochs a continent several hundred feet lower than now, the ocean reaching to Louisville and Iowa, with a subtropical climate prevailing over the lake region, the climate of Greenland and Alaska being as mild as that of southern Ohio at present.

(*b*) A preglacial epoch of gradual continental elevation, which culminated in the glacial epoch, when the climate of Ohio was similar to that of Greenland at present, and glaciers covered a large part of the surface down to the parallel of forty degrees.

(*c*) This period followed by another interval of continental subsidence characterized by a warmer climate and melting glaciers and by inland fresh-water seas filling the lake basins, in which were deposited the Erie and Champlain clays, sands and boulders.

(*d*) Another epoch of elevation which is still in progress.

The sheet of clay and boulders which was found directly overlying the polished surface of the rocks over so large a part of the state, now known under the name of till and boulder clay, he described under the general name of glacial drift, while the loose boulders which he found scattered indiscriminately over the surface, frequently resting on the fine stratified clays, were known under the name of iceberg drift.

If, he wrote, we restore in imagination this inland sea, which we have proved once filled the basin of the lakes, gradually displacing the retreating glaciers, we are inevitably led to a time in the history of this region when the southern shore of this sea was formed by the highlands of Ohio, etc., the northern shore a wall of ice resting on the hills of crystalline and trappean rocks about Lake Superior and Lake Huron.

From this ice wall masses must from time to time have been detached, just as they are now detached from the Humboldt glacier, and floated off southward with the current, bearing in their grasp sand, gravel and boulders—whatever composed the beach from which they sailed. Five hundred miles south they grounded upon the southern shore—the highlands of now western New York, Pennsylvania and Ohio, or the shallows of the prairie region of Indiana, Illinois and Iowa. There melting away and depositing their entire loads.

The loess, as one would naturally expect from the foregoing, was looked upon as the finer sediment deposited in the quiet waters of one of these inland seas, to which the icebergs had no access. The lake basins, with the exception of that of Lake Superior, were regarded as excavated by glacial action—thus agreeing with Logan.

The views of Orton, who succeeded Newberry as state geologist, were not widely different, and, briefly expressed, were as follows:

Threefold divisions of glacial time may be considered as demon-

strated: (1) An age of general elevation of northern land, accompanied by intense cold and the formation of extensive continental glaciers; (2) a general depression of the land, with the return of a milder climate; (3) a partial reelevation of the land and a partial return of the cold climate, producing local glaciers and icebergs.

E. T. Cox, while state geologist of Indiana, encountered phenomena in every way similar to those described by Newberry and Orton, and it is to be expected that his mode of accounting for the same would be somewhat similar. In his report of the conduct of the survey of that state (1869–79) he announced his acceptation of the general theory of glacial drift, as at that time understood, and conceived that the necessary climatic changes might be due to the relative position of land and water, and, possibly, a change in the course of the Gulf Stream. He could find, however, no evidence of a subsidence of the land to terminate the glacial period, nor could he find in Ohio, Indiana or Illinois anything to militate against the commencement of the glacial period in Tertiary times and its continuation until brought to a close by its own erosive force aided by atmospheric and meteorological conditions. By these combined agencies acting through time the mountain home of the glacier was cut down and a general leveling of the land took place. This suggestion that the glacial epoch worked out its own destruction through a process of leveling, whereby the altitudes which gave it birth were so far reduced that glaciers could no longer exist, is unique and, so far as the present writer is aware, original with Cox.

The organization in 1876 of a state geological survey of Wisconsin afforded Professor Chamberlin and his assistants opportunity for investigation of the drift phenomena of that state, and in the pages of his reports his views are distinctly formulated. He divided the glacial period into: (1) The terrace or fluviatile epoch, (2) Champlain or lacustrine epoch, (3) the second glacial epoch, (4) the interglacial epoch and (5) the first glacial epoch. This formal announcement of the possibility of two distinct periods of glaciation was here made for the first time, although, as before noted, Edward Hitchcock had at an earlier date suggested such a possibility.

Not content with a mere discussion of the glacial phenomena, Chamberlin considered also matters relating to the cause of glacial movement. The law of flowage he announced as being, in his opinion, similar to that of viscous fluids—this in accordance with the observations of Agassiz, Forbes, Tyndall and others. A later study of Greenland glaciers, as is well known, has caused him to change his views on this point.

In the third edition of his work on 'Acadian Geology,' which appeared in 1878, J. W. Dawson returned once more to a vigorous dis-

cussion of the problems of the ice age, and to register again his opposition to the views generally held by American geologists. Many of the arguments used closely resembled those of his former papers and may be reviewed here for the last time.

He regarded the phenomena of the boulder clay and drift in eastern America as due to the action of local glaciers, drift ice and the agency of cold northern currents. Against the theory of a universal glacier he again argued on the ground that such suppositions were not warranted by the facts.

> The temperate regions of North America could not be covered with a permanent mantle of ice under existing conditions of solar radiation; for, even if the whole were elevated into a tableland, its breadth would secure a sufficient summer heat to melt away the ice except from high mountain peaks.

For the supposition that such immense mountain chains existed and have disappeared, he found no warrant in geology, and for such an 'unexampled astronomical cause of refrigeration' as the earth's passing into a colder portion of space, he found no evidence in astronomy. He agreed with Lyell in regarding the theory of the varying eccentricity of the earth as expounded by Croll as insufficient; moreover, it seemed to him physically impossible that a sheet of ice, such as that supposed, could move over an uneven surface, striating it in directions uniform over vast areas and often different from the present inclination of the surface.

He was further influenced in his opinion by the work of Hopkins, who showed, apparently, that only the sliding motion of glaciers could polish or erode rock surfaces, and the internal changes in their mass—the result of weight—could have little or no effect. Glaciers, moreover, he argued could not have transported the boulders great distances and lodged them upon the hill tops, and the universal glacier would, moreover, have no gathering ground for its materials. The huge feldspar boulders from the Laurentide Hills, stranded at Montreal Mountain at a height of six hundred feet above the sea and from fifty to sixty miles further southwest, and which must have come from little, if any, greater elevation and from a direction nearly at right angles to that of the glacial striæ, were against the ice-sheet theory, as were also the large boulders scattered through the marine stratified clays and sands, and the occurrence of marine fossils in the lower part of the drift in the true till near Portland, Maine, and at various points on the St. Lawrence in Canada.

To substantially these views Dawson held to the very last. In his 'Ice Age in Canada' (1893) he is found still combating vigorously the idea that all northern Europe and America were covered by a *mer de glace* moving to the southward and outward to the sea, and which moved not only stones and clay to immense distances, but glaciated and

striated the whole surface. The glacial theory of Agassiz and others he described as having grown, until, like the imaginary glaciers themselves, it overspread the whole earth. He adopted, rather, what he called the moderate view of Sir Roderick Murchison and Sir Charles Lyell to the effect that Pleistocene subsidence and refrigeration produced a state of our continents in which the lower levels, and at certain periods even the tops of the higher hills, were submerged under water filled every season with heavy field ice formed on the surface of the sea, as at present in Smith's Sound, and also with abundant icebergs derived from glaciers descending from unsubmerged mountain districts. The later Pliocene, so far as Canada was concerned, he considered to be a period of continental elevation and probably of temperate climate.

Thus far the discussion relating to the ice period has been limited wholly to workers and areas east of the Mississippi River. In 1880 and 1882 J. D. Whitney, one time state geologist of California, issued his well-known work on climatic changes of later geological time, in which he discussed the occurrence of glaciers and their possible origin in the west, particularly in the region of the Great Basin. Whitney thought to be able to trace a period of warmth and heavy precipitation, followed by one of desiccation, but anticipated by one of cold and glaciation, the glaciers, however, being limited to the most elevated ranges of the Cordilleras. At the outset he announced himself as opposed to the 'wild and absurd ideas' that had prevailed regarding glaciation in the Sierras, and stated it as his belief that here, at least, ice had played but an extremely subordinate part as a glacial agent, though 'there is no doubt but that the great California range was once covered with grand glaciers, but little if at all inferior to those which now lend such a charm to the Swiss Alps.'

It was Whitney's opinion, further, that the geological importance of the ice sheet had been greatly exaggerated. It seemed to him beyond question that icebergs had played an important part in carrying and distributing the large angular boulders which in many places rest upon the surface in such a manner as to show that they could not have been placed in their present position by running water or by a general ice sheet.

He regarded it as evident enough that the climate of northeastern America during the glacial epoch was a period of greater precipitation than now, but that it was a period of intense cold he would not admit. Glaciation or a glacial period was due merely to increased precipitation. In order that such precipitation should take place, an increased evaporation from the land and water was necessary. This could be brought about only by a general increase of temperature. The amount of precipitation being sufficient, the production of glaciers would depend

upon temperature, which itself would be dependent upon local conditions, which again might, or might not, be due to elevation of land surfaces. His idea, in brief, was that while during the glacial epoch there might be over the entire globe a period of sufficient warmth to produce the desired evaporation, the precipitation would fall as rain or snow, according to the local uplift or depression. That the glaciers are now retreating in nearly every instance, he regarded as due, not so much to a change in climate, at least not to a gradual increase of temperature, but rather to a gradual decrease in the amount of annual precipitation.

In this connection, it may be mentioned that Whitney considered the movement of glacial ice due to water:

> Glacier ice is not simply ice, but a mixture of ice and water, and it is to the presence of the latter that the whole mass owes its flexibility. The larger the amount of water, other things being equal, the more easily the glacial mass moves. When the water increases so as to get the upper hand, the ice gives way with a rush and becomes an avalanche. . . . The extreme variation of the rate of motion of different glaciers coming down from the inland ice of Greenland is due to the different amounts of water which they have imbibed.

More recent observations than those quoted are familiar, and we may well stop here. That, at a period geologically not very remote, a vast sheet of ice and snow, with all the attributes of a modern glacier, or series of glaciers, covered the northeastern United States and eastern Canada, that this sheet advanced, retreated, and again advanced, and finally utterly disappeared, is the commonly, though not universally, accepted view. The causes which led up to this condition are still problematical. Whether due to cold from increased elevation, as taught by Dana, to astronomical causes, as taught by Croll, or merely to an increase in precipitation, as argued by Whitney, or to a combination of any or all of these causes, is the great problem awaiting solution, if solution is possible on other than a theoretical basis. Chamberlin, the Dawsons, Gilbert, Hall, the Hitchcocks, Lewis, Mather, Newberry, Salisbury, Upham, Winchell, Wright and a score of others have made us acquainted with the physical characteristics of drift deposits and their geographic distribution, but the first-named alone, among Americans, has put forward a satisfactory working hypothesis as to the cause of glacial motion.

Leaving out of consideration Peter Dobson, whose views were not pushed to their legitimate conclusions, the world at large must credit Louis Agassiz, born in Switzerland, but an American by adoption, with being the great promoter and, perhaps, originator of the glacial hypothesis as it exists to-day. His method of procedure, it is interesting to note, consisted in applying what one of our prominent geologists has slightingly referred to as the principle of prolonging the harmless and undestructive rate of geological change of to-day backwards into the deep past.

THE EARLIEST PREDECESSORS OF COPERNICUS

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THE first glimmerings of perception that presage the discovery of great truths, whether coeval with, or long antecedent to complete apprehension, possess for most minds a fascinating interest. Whether it be abstract ideas, epoch-making inventions or discoveries of fundamental laws, such as gravitation or evolution, matters not; the names of those who have contributed largely toward intellectual progress, even if they fell short of the whole truth—if they merely prepared the way for final discovery—become universally reverenced, or acquire at least a romantic interest, by virtue of their heraldry. In natural science, as elsewhere, it is right and fitting that a high place be reserved for the advance prophets who have preceded the great expounders of Nature's truths.

By common consent of mankind, the Copernican cosmogony ranks as one of the immortal triumphs of genius, whence it follows that not only the monk of Frauenburg, and the scarcely less famous defenders of his theory, but also his remote predecessors, are entitled to an exalted position amongst those favored mortals who have been permitted, in one age or another, to wrest from Nature the solution of her deepest mysteries. Yet because of the absolute originality of the great sixteenth-century astronomer's discovery; because, furthermore, the anticipators of his theory failed to convert the world to their belief; and because Copernicus himself knew nothing of his real predecessors, it has come about that too little credit is commonly bestowed upon ancient forerunners of our modern system.

There is abundant and undisputed testimony to show that in the minds of at least two astronomers prior to the Christian era, namely, Aristarchus of Samos, and Seleuchus the Chaldæan, the essential features of our modern system were clearly recognized, and elaborated not only into a working hypothesis, but into a valid explanation of the universe. It is impossible to view this achievement in its manifold relations to human progress, without being impressed with the pathetic and remarkable fact that a ray of divine intelligence should have shone forth for an instant in that far-off period, kindling here and there a momentary spark; sparks that, although revealing the true order of the cosmos, were not finally to illumine the world until after the lapse of centuries.

No demonstration is necessary to show that the beginning of astronomical knowledge is a subject in which all intelligent persons

are interested. Indeed, precisely this sort of interest is recognized by Professor Simon Newcomb as one of the characteristics of modern science. The opening words of his presidential address at the St. Louis Congress tell us that "among the tendencies characteristic of the science of our day is one toward laying greater stress on questions of the beginnings of things, and regarding a knowledge of the laws of development of any object of study as necessary to its complete understanding in the form in which we find it."[1] But interest alone is scarcely sufficient to overcome the difficulties which beset the general reader in quest of information, owing to the exceedingly scattered, and more or less special nature of the literature. The absence, too, of a popular conspectus which treats in satisfactory manner of the origin of the heliocentric hypothesis, is to be regretted.[2] Hence the present article has been prepared with the idea of affording those who may be inquisitive as to the sources of information with the means of orienting themselves.

In order to keep the relations of the founders of different cosmical systems firmly in mind, the distinguishing features of the latter may be pointed out, with indication of the most prominent names associated with each. Four distinct systems may be recognized, according as the center of the universe is supposed be occupied by (1) a central fire, other than our sun (Philolaus the Pythagorean); (2) by a stationary earth about which the heavens revolve (Eudoxus of Cnidos, Ptolemy, Tycho Brahé); (3) by an earth rotating upon its axis, but otherwise immovable (Heraclides of Pontus); and (4) by the sun, about which the entire planetary system revolves (Aristarchus, Copernicus). As thus outlined, the transition between the third and fourth of these systems appears at first sight abrupt, but examination of the views of Heraclides shows that he too recognized the competency of the modern system as a working hypothesis, and thus helped prepare the way for its rigorous adoption. It is evident, also, that a somewhat remote

[1] 'The Evolution of the Scientific Investigator' (opening address of the president of the International Congress of Arts and Science, at the St. Louis Exposition), *Science*, Vol. XX., p. 385.

[2] The treatises by Berry ('A Short History of Astronomy,' London, 1898), and Miss Agnes Clerke (in Appleton's 'Concise Knowledge Library,' New York, 1898), contain but a bare mention of the prior establishment of the Tychonian and Copernican systems amongst the Greeks. Even the elaborate work of Sir George Cornewall Lewis ('An Historical Survey of the Astronomy of the Ancients,' London, 1862) can hardly be said to do justice to the evolution of these systems. The French reading public is more fortunate than ours, having a goodly number of popular works at its disposal. Two only need be mentioned here, each of them being provided with ample bibliographical references. These are: Thirion, J., 'L'evolution de l'astronomie chez les Grecs' (Brussels, 1900); and Bonnel, J. F., 'Étude sur l'histoire de l'astronomie: la Découverte du double Mouvement de la Terre' (Tours, 1886).

connection can be traced between the last of these systems and the first, a correspondence which did not escape notice by Copernicus himself.[3]

Familiar as Copernicus undoubtedly was with Pythagorean doctrines, how are we to explain his silence regarding the system of Aristarchus? The answer lies in the fact that he never had access to the writings of Archimedes, which furnish our chief information in this matter; indeed he could not, for the reason that the *editio princeps* was not given to the world until the year following his death in 1543.

For a succinct statement of the views of Aristarchus, as reported in the *Arenarius* of the famous Syracusan, one may refer to an article by Professor Holden in an earlier number of POPULAR SCIENCE MONTHLY (April, 1904). The original text of the passages, both in the *Arenarius* and in Copernicus relating to them, together with a variety of precious documents extracted from ancient authors, is appended to the anniversary memoir of Professor Schiaparelli, prepared in honor of the fourth centenary of the birth of Copernicus.[4]

At the same time it must be admitted as at least curious that the brief sentence in Plutarch (*de Placitis Philosophorum,* II., 24), in which Aristarchus is represented as having reckoned the sun amongst the category of fixed stars, and to have conceived of the earth revolving around it, should have passed altogether unnoticed by Copernicus. Almost the identical words are repeated by Stobæus in his *Eclogæ Physicæ,* and in the distorted abridgment of Plutarch's treatise which passes under the name of *Historia Philosophica,* often erroneously attributed to Galen; but we must suppose that none of these statements attracted the attention of Copernicus, even if he was aware of their existence. The same remark applies to passages concerning Aristarchus which occur elsewhere in Plutarch and amongst other authors, fortunately in considerable number. Those desirous of consulting them

[3] The system of Philolaus is twice mentioned by Copernicus in his famous work, 'De Revolutionibus Orbium Cælestium,' first in the dedicatory epistle to Pope Paul III., and again in the fifth chapter of Book I. Upon these passages, which gave rise to heated discussions a few decades after the death of Copernicus, Professor Schiaparelli comments as follows:

'Neppure qui è possibile inferire, che nella mente di Copernico il sistema di Filolao fosse il sistema eliocentrico. Anzi, le caute e indeterminate espressioni . . . mostrano che le parole di Plutarco più sopra citate non gli sembravano abbastanza decisive per invocare l'autorità di Filolao in favore del sistema da lui [*i. e.*, Copernicus] propugnato."— *I Precursori di Copernico,* etc., p. 9, note 20.

[4] Published in the *Memoirs of the Royal Lombardy Institute,* Vol. XII., and also in the Publications of the Milan Observatory, No. 3, 1873. A German translation by Curtze exists under the title of 'Die Vorläufer des Copernicus im Alterthum' (Leipzig, 1876). Three other invaluable historical memoirs by the same author have appeared in the Lombardy Memoirs, the latest one (1898) dealing in consummate manner with the 'Origin of the Heliocentric Planetary System amongst the Greeks.' These contributions are absolutely indispensable for students.

in the original, and of comparing the opinions of learned critics, will readily be directed to them by the special literature.[5]

Contenting ourselves with these brief literary indications, we may pass on to the more intricate questions relating to the predecessors of Aristarchus, and the influence of Pythagorean views upon later thinkers. As has been previously remarked, Heraclides Ponticus acknowledged that the heliocentric theory provided an adequate explanation of celestial phenomena, and even approached so nearly to modern ideas as to maintain the revolution of Mercury and Venus about the sun. This we know from the testimony of numerous authors, chiefly Roman, amongst whom Heraclides was held in high regard. Of interest is the passage in Simplicius ('Commentary on Aristotle's *de Cœlo*,' Karsten's edition, p. 232), which shows Heraclides' correct apprehension of the causes determining the difference in length between the sidereal and ordinary day of twenty-four hours. We are informed, however, by Plutarch (*Plac. Philos.*, III., 13) and later writers (*e. g.*, Simplicius, Hippolytus, Proclus, Chalcidius, and especially Vitruvius and Terentius Varro) that although Heraclides of Pontus and Ecphantus the Pythagorean believed the earth to turn upon its axis from west to east, they distinctly denied to it a movement of translation through space. It is clear also from Aristotle that it was no unheard-of thing to explain the apparent diurnal motion of the heavens upon the hypothesis of the earth's rotation. Plato appears to have accepted this idea as the starting-point of his system, complicated as it was with superadded mechanism. But the great Athenian appears to have elaborated his cosmical theory more as a speculative abstraction than as an orderly induction from observed facts, and it was easy to explain the discrepancy of the latter as due to false appearances.

It has been claimed on the authority of Theophrastus, as reported by Plutarch and Aristotle, that Plato repented in his old age at having placed the earth at the center or 'altar' of the universe, this being deemed too sacred a position for it to occupy (Plutarch, *Plat. Quæst.*, VIII. 1; Aristotle, *de Cœlo*, II. 13, 3). But this is very far from indicating that the heliocentric theory ever fully shaped itself in his mind, although one sees that it required merely a combination of his views and those of the Pythagorean school to arrive at a cosmical

[5] Besides the writings of Schiaparelli above mentioned, one should not fail to consult H. Martin's works, especially his 'Etudes sur le Timée de Platon,' Vol. II. (Paris, 1841), and Paul Tannery's 'Recherches sur l'Histoire de l'Astronomie ancienne' (Paris, 1893). The fourth essay in Bergk's 'Abhandlungen zur Geschichte der griechischen Philosophie und Astronomie' (Leipzig, 1883) is devoted to Aristarchus. The older work of Schaubach contains some rather adverse estimates, no longer considered tenable. On Pythagorean doctrines, one of the most critical essays in English is by George Grote: 'Plato's Doctrine respecting the Rotation of the Earth, and Aristotle's Comment upon that Doctrine' (London, 1860).

system identical with that which we call Copernican. Moreover, if we may trust to a somewhat obscure statement in Simplicius, there lived in the time of Alexander the Great an individual whose name we know not, but who actually did effect a combination of these ideas, and who is therefore worthily entitled to rank as a predecessor of Copernicus. Whether the heliocentric conception was ever presented to Aristarchus in concrete form, or was independently excogitated by him, we are without information; but it is impossible that his mind should not have received some fertile stimulus from the ideas already extant concerning the earth's revolution and rotation. Indeed, the way had been fairly prepared for a realization of the Copernican system; and as a matter of fact it was easier to arrive at this conception in the time of Aristarchus than subsequently, when the scheme of planetary movements had become hopelessly obscured through the invention, by Apollonius of Perga, of eccentrics and epicycles. The transition from Philolaus to Aristarchus is natural and easy as compared with the truly Herculean feat performed by Copernicus, who had first to clear away heaps of Augean refuse before the truth could again become manifest.

A melancholy interest in the fate of Aristarchus bids one inquire the reasons which prevented his theory from obtaining foothold. So far as history tells, it found but a solitary champion in the person of Seleuchus,[6] who flourished half a century later than Aristarchus. To Archimedes, and presumably to contemporary mathematicians and philosophers, the insuperable objection to this system consisted in its stationing the fixed stars at an infinite distance from the earth. Moreover, as witness the clamant protests against the Sage of Athens—to say nothing of the witty caricatures of him in the 'Clouds'—followed in the end by his martyrdom; and as witness the charges preferred against Aristarchus by Cleanthes, any dislodgment of the earth from its sacred position in the 'hearth of the Universe' was tainted with suspicion of impiety. And when afterwards the Ptolemaic mechanism was introduced, blocking with its devices the brilliant conception of Aristarchus, fourteen centuries were required to roll by before this useless débris could be swept away.

Possibly yet other circumstances conspired to hinder the acceptance of the heliocentric system, the nature of which can not now be ascertained, any more than can the reasons which first carried conviction of its truth. But this much is clear, there can be a tragic history of ideas no less than of individuals; and in meditating on the fate of the many 'struck eagles' of the pagan world, who soared loftily even where we now stumble, one is reminded of that beautiful simile of Byron, which concludes in deepest pathos:

> Such is the aspect of this shore;
> 'Tis Greece, but living Greece no more!
> . . . . . .

[6] *Cf.* Ruge, S., 'Der Chaldäer Seleukos,' Dresden, 1865.

THE GREATEST NEED IN RESEARCH

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ANY student of social progress might learn a useful lesson if he would attend a convention of the National Educational Association, which, in the general character of its work, is typical of the numberless educational organizations existing among us. He would find the most complicated questions of educational procedure being discussed by a body of men of the most divergent interests, training and experience—by prominent public officials who may boast of the fact that they were not trained in the schools themselves; by college presidents and professors in technical departments who have never given an hour's thought to the principles of education; by normal school principals and school superintendents, who devote all their energies to executive details; and in addition to these one may listen to dogmatic opinions regarding studies and methods from editors of the secular and professional press, parents and citizens, merchant princes, bankers, lawyers, physicians, ministers—any one who has attracted attention in any field of practical activity is likely to be invited to give teachers directions as to how they should 'train up the rising generation.' The sort of person who will be least in evidence at the convention is he who is carefully investigating some particular problem of education according to scientific methods. Program makers usually do not wish 'theoretical' or 'laboratory' papers; they want something 'spicy,' 'concrete,' 'practical,' 'common sense.' Study the programs of educational gatherings, and note how largely they are devoted to the exploitation of mere opinion based upon incidental and shallow observation. One does not often hear a governor, say, or a college president, or a professor of Greek, or an editor of a daily paper, instructing physicians regarding the nature of disease and how it should be treated; but such persons will often dogmatically lay down the laws to teachers respecting educational values and methods. They justify themselves on the ground of superior 'common sense'; specialists, they say, men who devise ways to overcome the universal tendency to interpret everything in the light of individual experience and preconceptions, so that they may examine the phenomena in a special field with an eye single to the truth—such men are not generally favored by the gods with well-balanced minds, and only the man who knows a little of

everything, but nothing particular about the subject he discusses, is competent to give sane advice as to the rearing of the young.

In almost every field of human activity outside of education the expert alone, who has become possessed of special knowledge, can gain a hearing and a following. Improvement along most lines of human endeavor has been achieved only by clearing the way of the 'common-sense' men; they always block rational progress, for they never penetrate beneath the surface of any problem. When any particular department of social activity is largely dominated by such men it must certainly lag behind those departments where fact is esteemed more highly than mere fancy, and where searching for truth is more prominent than the promulgation of individual opinion. Little was understood of the laws of nature until people with scientific interests abandoned the 'common-sense' notions of the universe which were current among men until within recent times, and devoted themselves to tracking out these laws without preconception or bias. The mind of the 'common-sense' man, as we find him in daily life, functions only for the purpose of getting his own prejudices adopted by his fellows. He is not fitted, intellectually or temperamentally, to discover the deeper-lying truths in any field. He is a partisan, an advocate, not a truth-seeker; and he must be ejected from every scientific camp before advance can be made.

Consider what would be the situation to-day in physics, or chemistry, or electricity, or medicine, or mechanics, or law, if every aspiring person in the community could set himself up as an authority in any of these fields, and he should be given a chance to disseminate his views. In these departments a man who poses as an authority without having mastered at least the rudiments of the subject he treats is cast into outer darkness without ceremony or apology; but he may be welcomed by teachers if his rhetoric is pleasing, and he claims fellowship with the 'common-sense' tribe, or if he has a reputation for greatness in some sphere of action, though quite remote from education. Educational people have had a liking beyond other persons, perhaps, for generalities and commonplaces and oratory and hero-worship; science has not been emotional enough; it has required too precise thinking, and to appreciate it has involved too elaborate training.

But we are beginning to see evidences of brighter days ahead. The scientific temper is beginning to show itself in those who treat of education. There is developing in some quarters discontent with the methods that have been pursued in discussing questions of education, and we are just ready to enter upon an era of educational investigation in accord with strict scientific method. Men are coming to realize that traditional educational dogmas are, in considerable part surely, founded upon the shifting sands. In many other fields there would be no rest

or peace until all this folk-lore had been subjected to scientific test; and it looks now as if the educational world was on the eve of a period of stress and struggle in the effort to examine the character of the foundations upon which all our theory and practise are built. The National Educational Association has established a fund for research; a society for the scientific study of education has been formed in our country; two or three of the universities have established chairs of educational research; a number of men have put themselves into training for the new work; and these are but preliminary signs of the impending revolution in the treatment of education.

And we shall need to start practically at the beginning in our research. Much, perhaps most, of contemporary educational opinion is in dispute, and we can not be certain where the truth lies. Take such a simple matter as the teaching of the three R's; while we are agreed that every child should gain some familiarity with these branches, yet we have the most diverse theories as to at what age we should introduce him to them, just what he should get from each, whether they should be acquired in isolation or correlated with other branches, how they may be most economically mastered, and so on *ad libitum*. For the asking, and even without it, we can get all sorts of opinions on these problems from all sorts of persons from college presidents up and down; but who among all these has resolved any one of these well-nigh infinitely complex questions into its elements, as scientific procedure demands, and observed it under varying conditions, so that its precise value could be determined? 'Common sense' does not realize that these problems are complex; it catches some shallow, immaterial aspect of any situation, and jumps to the easiest conclusion, missing most of the vital factors of the problem. Much of our traditional educational theory has been established in this way; it is in some such condition to-day, as natural science was when Bacon began applying exact methods to the study of natural phenomena. We have a great deal of hearsay knowledge about human development; but when one attempts to administer educational forces with precision, certainty and efficiency, he realizes how much guesswork there is in current pedagogical opinion. Science is only just beginning to touch questions of development at all; men in all fields of living nature have been concerned primarily with mature things, analyzing and dissecting and classifying. Even medicine has given us little of value regarding the healthful physical development of a human being. We have almost no precise knowledge respecting problems of food, clothing, sleep, exercise, the effects of school-life and the like upon an individual at different periods in his development. We have an unlimited body of conflicting lay opinion upon these matters, and a considerable body of conflicting expert (?) opinion as well; but if a layman who has children to bring up, say,

should consult all these opinions in the hope of getting some aid in his task, he would be more likely to be confused than enlightened. Take the current standard literature on the feeding of children, for instance, and you will find exactly opposite opinions expressed upon the most vital matters by equally 'eminent authorities'; and you will discover that we have but little on this subject which has been worked out with due regard to scientific accuracy. The trouble is that a man who may be an authority in some phase of the malfunctioning of the adult organism, but who has made no exact studies upon the developing organism, does not hesitate to dogmatize about the latter in the light of his experience with the former. While doubtless he may be partially right in his views, still what we now need is precision as a result of special research in the field of human *development,* physical as well as intellectual and moral. Here is the great necessity and the great opportunity for research.

Doubtless one, and it may be the principal, reason why research in education has lagged far behind that in many other fields is because the practical work of instruction has absorbed the attention and energies of educators. There has been so much to do in carrying out the conventional educational régime that men have not had leisure to even investigate the foundations of this régime. Teachers are always confronted by situations where something *must be done immediately,* and they are compelled to act in view of what seems traditionally best. It is not permitted them to doubt the validity of the principles transmitted to them, for to doubt is to become static, and the great public demands action of a clearly obvious nature. Then naturally, of course, when the teacher acts on a principle through necessity, he becomes its exponent and defendant, and easily convinces himself that it is sound, and in this way he helps to pass it on as truth to his associates. Heretofore there has been no body of men in education, as there has been in other fields, who have been sheltered from the urgency of people of utilitarian impulses and needs, and who have been given leisure to work out problems without feeling that principles and rules of practical value must be elaborated at once right out of hand. In physics and chemistry and agriculture and medicine and other departments there are men at work who devote all their time and energies to original investigation, and they are not coerced into forming hasty opinions in order to gratify a public demand; but it is quite different in education. The supreme need to-day in this latter department is the development of a body of investigators who will be recognized as such, and who will be protected from the importunities of the practical people about them. Taken as a whole, the universities, some of which make reasonably liberal provision for research in the physical sciences, agriculture, medicine and the like, make no provision whatever for research in education.

The majority of them yield to the general clamor for something immediately serviceable in reference to teaching, and so they engage two or three instructors who are expected to give themselves entirely to the work of instruction, and to enlisting the support of the teachers in their several communities for their respective universities. The situation would not be so much in need of remedying if the normal schools were making any progress in research, but they too are engrossed with immediately practical affairs. They must look to other institutions—properly the universities—for new light, and they will then spread it among the people.

It is worthy of remark that a country which keenly appreciates the necessity of scientific experimentation in agriculture, and carries it on very effectively, should not think it needful to provide for similar experimentation in the care and culture of human beings during the formative period. Some one may ask whether the National Bureau of Education is not an investigating institution; and the answer is that is not intended to make, nor is it making, the slightest contribution to educational science, except in so far as the gathering of statistics regarding school attendance, the wages of teachers, the progress of new studies, as manual training and nature study and the like, may be found to bear in some way upon educational theory. It can not take the initiative in any research; it can simply report what is being done. The men who manage our educational finances have evidently imagined that since so many people are engaged in educational work they would be constantly pushing forward into the unknown, ever widening the boundaries of knowledge about human nature and the means of influencing it most effectively and economically. But it is just as reasonable to assume that practical farmers will continually develop the science of agriculture without experiment stations, or that practical doctors will develop the science of medicine without research laboratories, as to assume that practical superintendents and principals and class-room teachers will develop the science of education without special schools for investigation.

MAKING GEOGRAPHY WHILE YOU WAIT

By THOMAS H. MACBRIDE,
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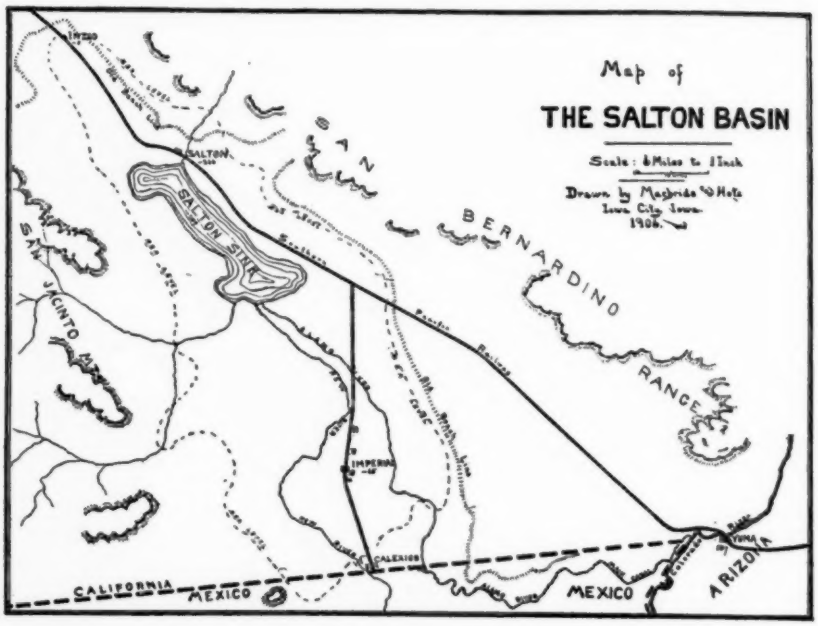
THOSE who read the daily newspaper, and the number of such is confessedly great, have no doubt been more than once of late mildly excited by certain sensational despatches from California, despatches intimating that certain large portions of that much-advertised commonwealth are actually rapidly disappearing from the sight and touch of men. It is reported that a large valley in the southernmost portion of the state is vanishing; has been assaulted by the sea; volcanoes are breaking up the solid ground beneath and the gray ocean is coming in to cover up the universal ruin.

Now something might be said in apology for the real-estate men of California who let go their holding in presence of assaults like these; and if it can be shown that any considerable bit of realty is actually disappearing and escaping, the violence of the natural agency responsible becomes a matter of probability, at least.

But it must be admitted that for the sensational stories referred to there is a certain basis of fact. In the Salton desert of southern California, where less than two years ago the traveler on the Southern Pacific railway saw only a wide vista of drifting sand, he now may skim along for miles beside a spreading sea, a sea that deepens from day to day and widens every moment. Within less than two years over many square miles, the whole face of the country has been changed. Railroads have been whelmed, stations and houses and factories lost in one encroaching flood; gulls and cranes run along a level beach where but a few months since the sage-hen nested, or the wild rabbits hid in sandy burrows. Surely, whether Neptune and Vulcan are busy or not, something has happened, and the passing traveler would like to know.

If we consult a map of the region in question and at the same time study the levels of the country, even as cited in the railway folder, though these are inexact, we shall speedily discover that the old Salton desert is indeed a basin, a basin of remarkable depth for its expanse, and wonderful in many ways. To make the case as simple as possible, the altitudes of points on the accompanying map are indicated. It appears, for instance, that the altitude of the station at Yuma, just east of the basin, is one hundred and thirty-seven feet. While the height of Salton station, about the middle of the valley, now whelmed,

is actually two hundred and sixty-two feet below the level of the sea, say at the head of the Gulf of Lower California, about a hundred miles away! Indio, another railway station about one hundred and twenty-five miles west of Yuma and about twenty-five west of Salton, is almost at sea-level, so that it is evident that we have here a great depression not only below the Colorado which passes Yuma, but actually away below the



level of the sea. The extent of country actually below sea-level is included by the dotted lines upon our map. Now it is evident that if the volcanoes should once conclude to open up a way for oceanic waters, the sea might easily take possession of the Salton basin as the newspapers say; but have they done it?

It has been noted that the Colorado River at Yuma is far above the bottom of our basin, and it skirts along our southeast border to the gulf all the way, of course, far above the valley. In fact, with respect to the basin and its sloping sides, the river occupies exactly the position of a great irrigating main carried along some hillside above waiting fields. Some years ago enterprising men who saw the situation, realizing that large areas of the basin were not sand at all, but the finest sort of fertile alluvial soil, began to use this great natural main by constructing a secondary, carrying the waters of the Colorado out to the south part of our basin, near the Mexican boundary, where it was easy to bring under water some 100,000 acres of beautiful land. This artificial channel should bring part of the water of the river to certain old river channels

emptying, or rather opening, into the lower part of the Salton basin. Recently, for the sake of wider service, new intakes from the Colorado have been opened. All went well until some eighteen months since, when the Colorado, flushed with flood, rose suddenly twenty feet or more above its usual tide and forthwith, instead of hurrying its waters southward to the gulf, began to pour them down the old abandoned river channels into the Salton sink. The fall to the gulf is only one hundred and thirty-seven feet, while the fall to the basin is, as we have seen, nearly four hundred and the distance not much greater! So there you are; and no earthquakes, volcanoes or sea-water needed in the least!

The whole situation and topography are interesting in the extreme. The geologist will tell you that all the region hereabouts is slowly rising; that once upon a time the head of the Gulf of California was farther north, away north of Yuma, in fact, and received the Colorado there, perhaps where the United States government is now building the new dam; and more, an arm of the gulf extended away west nearly to where now is Indio; and in those days the mud of the stream was deposited farther and farther out in the sea, forming an estuarine deposit, filling up the sea, while, upon the rising bottom, channels of the river ran carrying the mud farther and farther until finally the part of the sea toward Indio was cut off entirely from what now is called the Gulf. The part of the ocean thus isolated presently dried up and left the Salton basin, a salt desert by reason of the evaporated sea-water; and now again, though filled with the fresh water of the river, the wide-forming lake is salt once more in memory of its old-time history.

The botanist too finds curious confirmation of our story. All about the Salton valley, as near Indio and the Palm springs farther west are curious isolated groves of palms, palms of peculiar sort, the *Washingtonia,* in fact, now commonly planted in south California cities. But *Washingtonia* should stand by the sea, as the palms of Florida do, run down the shores of the California Gulf—and so these isolated groves are but the remnant of a tropic flora, once rich no doubt, that all but perished with the drying of the old Salton Sea of which geology tells. The old sea was a fact. Part of this beach is yet to be discovered, as is shown upon our map, and no doubt its course might be traced more widely still; its sands along the old-time eastern shore are still blowing about in dunes.

The waters of the Colorado, if allowed their present course, will no doubt bring back conditions of climate long gone by. Already railroad men declare the air too moist. If so, would the palms again extend their sway along the shores and would tropic verdure once more make the bordering mountains green? Who knows?

EXTRA DIGITS AND DIGITAL REDUCTIONS

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ALTHOUGH the mammalian extremities are nicely adapted by their structure to the functions they perform, the number of digits frequently varies from the normal. Moreover, different degrees of digital reduction may be observed in the extremities of animals whose habits are apparently identical. It is generally recognized that the digits of many mammals have been reduced to adapt the foot to rapid locomotion, but the evidence is chiefly circumstantial. In the present paper the writer will attempt to reconcile the various theories accounting for supernumerary digits, to call attention to certain evidences of reversion which may be daily observed, and to point out some little recognized factors concerned in the evolution of the mammalian foot.

We may assume that the primitive and typical mammalian foot was pentadactyl, in spite of Bardeleben's contention that the progenitors of the mammalia possessed not five but seven digits. Bardeleben's assumption was based upon the observation that certain mammals, the whale, for example, have more than five digits; that among five-toed forms six and seven digits occasionally occur; and that in many species small cartilages are present on each side of the hand and foot. These cartilages Bardeleben regards as digital rudiments, and the occurrence of extra digits is explained by him as reversion, a 'turning back' through heredity, to ancestral conditions. Unfortunately, the facts do not support this beautiful theory. Paleontology tells us that the forerunners of the mammalia possessed only five toes. Embryology has shown that the sixth digit of the whale, and the cartilages which Bardeleben supposes to be digital rudiments, develop secondarily some time after the typical five digits have appeared. Finally, observations have proved that the extra digits which occur in polydactylism do not develop from Bardeleben's 'digital rudiments,' but originate in an entirely different manner. We may, therefore, assume that the primitive mammalian foot was pentadactyl, and this being so, the occurrence of six or seven digits on a foot normally five-toed can not be attributed to reversion, unless we assume with Albrecht that it is reversion to the many-rayed fins of the Elasmobranch fishes, an absurd supposition. Such cases of polydactylism are, nevertheless, of frequent occurrence on the appendages of man and the cat. They have been explained as due to bifurcations or duplications of one of the typical five digits. Dissections show that this is really the case, for, though the skeletal

elements are often distinct from each other, muscle tendons and nerves are bifid, and in many cases the bones of the extra digit are more or less closely united to those of a normal toe. The question next arises as to whether these digital bifurcations are due to external influences or to internal variations of the germ plasm. Ahlfeld has observed that digital duplications may be caused 'in utero' by pressure from the thread-like outgrowths of the amnion. He attempted to make this explanation cover all cases of polydactylism, but there are several serious objections. In the first place, the extra digits generally occur on both hands or on both feet, often on all four extremities (Fig. 1, *A–D*). The middle digits, moreover, are not generally affected, but the duplication is chiefly of the first and fifth. Finally, and most important, the abnormality is strongly inherited and may increase in degree during successive generations. Thus Fackenheim cites the case of a woman born of normal parents. She had the little finger duplicated on each hand. Of two sons, one inherited the mother's extra fingers and the other had besides extra small-toes on both feet. Of eight children, three were normal, three had six toes and two had six fingers on both right and left extremities. In three succeeding generations the abnormality appeared, now on the hands, now on the feet, and in two cases on all *four* extremities; in two cases *seven* toes were present on both feet.

Similar observations have been made by Poulton and Torrey in families of cats. It is evident that extra digits produced by the chance pressure of amniotic threads would not be inherited, and that such chance pressure would certainly affect now one digit, now another; whereas, we have seen that the first and fifth digits are chiefly affected. Of twelve cases studied by the writer all were of the latter type.

It will be observed in Fig. 1, *A–D*, which represent the extremities of one child, that the fifth digit is affected differently in each case. In fact, it has been pointed out that occasionally no extra digit may be produced, that the first or fifth digit may simply be abnormally large. These facts, together with the frequent inheritance of the extra digits, show that we have to do here with variations of the germplasm. The first digit of man has been modified, and the fifth slightly reduced. Variation most often affects organs whose structure has been recently changed, and the variation or duplication of these digits might be naturally expected.

We are warranted, then, in assuming that the abnormal occurrence of six or seven digits on the five-toed extremity is not due to reversion. They are rather duplications of the normal digits, produced either by external influences or, more frequently, by germinal variation.

As the five-toed extremity is the primitive type of mammalian foot it is but natural to conclude that the appendage with less than five toes has lost some of the original number of evolutionary changes. The

circumstantial proofs of such reductions are too well known to require more than a brief statement. Rudiments supposed to represent the absent first digit are found in the pes of the dog and the manus of the pig (Figs, 2, *A*, 3, *C*). The feet of sheep and cattle exhibit pairs of vestigial bones and hoofs, called the rudiments of digits 2 and 5; the splint bones of the horse are believed to be the vestiges of the second and fourth digits. These rudiments are often better developed in the embryo than in the adult. Thus of the dog's hallux only the upper part of the metatarsal bone remains. According to Bonnet, all the skeletal parts of this digit are formed in the embryo. The second and fifth digits of the sheep, represented by mere vestiges of the phalanges, are fully developed in the lamb. The foot of the adult horse shows only the metacarpals and metatarsals of digits 2 and 4; but in the embryo the writer has observed two cartilaginous phalanges on the metacarpal bones. Paleontology completes the ring of circumstantial evi-

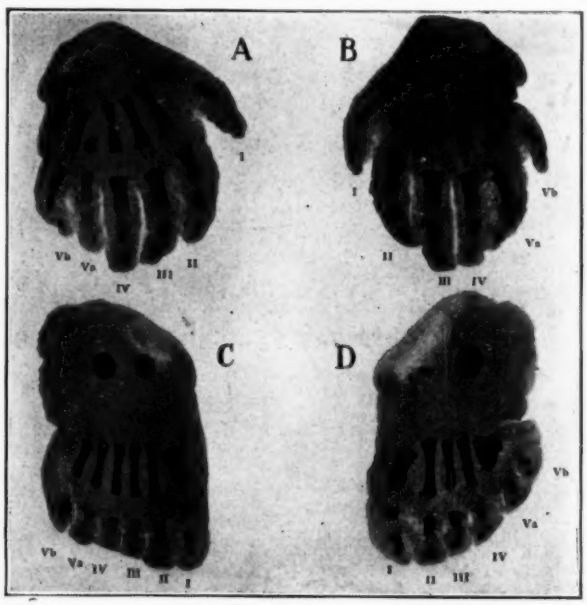


FIG. 1. X-ray photographs of a child's extremities showing duplication of the fifth digit in both hands and both feet. *Va*, *Vb*, the digits produced by duplication.

dence by showing us that the ancestors of the swine had five instead of four toes and that the forerunners of the ruminants and the Equidæ had three, four or five functional digits.

The question now arises as to whether the occurrence of *extra* digits on extremities normally possessing less than five toes is due to duplication, as in pentadactyl animals, or are the extra digits developed from the rudimentary structures we have described? If it can be shown that the supernumerary toes are due to reversion, we have no longer

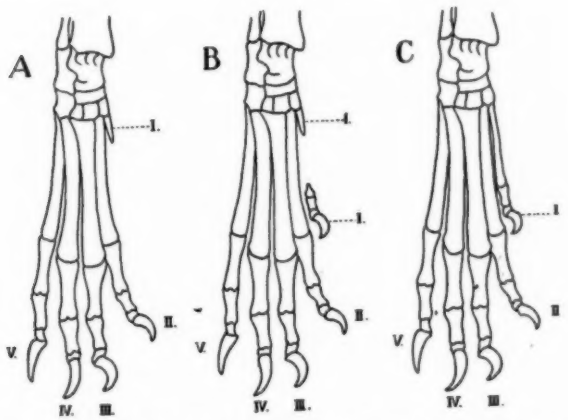


FIG. 2. A series showing the normal and polydactyl structure of the skeletal elements in the pes of the dog. *A*, normal pes with the rudimentary metatarsal bone of digit I.; *B*, a polydactyl pes, the hallux represented by two phalanges and the distal end of the metatarsal bone; *C*, polydactyl pes with hallux (I.) completely developed.

circumstantial evidence, but direct proof that the extremities of the ungulate have been derived by evolution from a five-toed type. This is an important point, but one about which investigators have been at variance. Bardeleben, Kollman, Marsh, Blanc and others recognize all cases of polydactylism as due to reversion. Gegenbaur warns us against such general conclusions, but admits that the extra digits sometimes found on the extremities of the horse are developed from the digital rudiments. Weismann, Bateson and Wilson ascribe all such abnormalities to germinal variation. But germinal variation may affect the rudimentary as well as the functional digits; if through such variation the supposed rudiment of a thumb develops into a digit with two phalanges, germinal variation and reversion are one and the same thing.

To attempt to reconcile the conflicting statements of various investigators the writer has made a comparative study of polydactylism in mammals normally possessing less than five toes. It was found that in the majority of cases the extra digits *are* developed from the so-called digital rudiments. This is most frequently observed in the pes of the

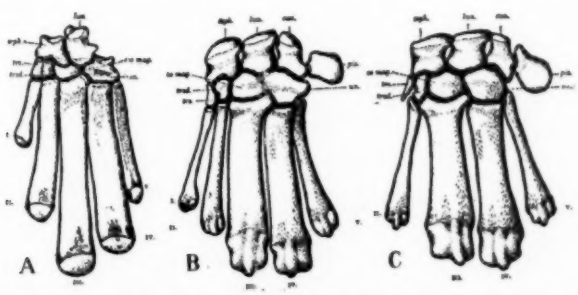


FIG. 3. A series to show the reversion of the pig's manus to the pentadactyl condition. *A*, carpals and metacarpals of the fossil Ancodus; *B*, of a polydactyl pig; *C*, of a normal pig. *I.–V.*, first to fifth digits; *trz.*, trapezium, the carpal element of the pollex.

dog. Normally the hallux, or great toe, is represented only by the proximal end of the metatarsal bone (Fig. 2, *A*). Not infrequently, a claw and one or two phalangeal bones may appear at the point where the hallux should be; occasionally the distal end of the metatarsal bone is also represented, and sometimes a complete digit with all the bones and articulations of a functional hallux may be developed. Such cases, which may be regarded with certainty as reversions to the five-toed type of foot, occur not rarely on the pes of the Scotch collie, St. Bernard and Newfoundland (Fig. 2, *B, C*).

In the foot of the pig the hallux is gone and the pollex is normally represented by a small carpal rudiment (Fig. 3, *C*). A small pollex was, however, present in the manus of *Ancodus,* one of the fossil swine

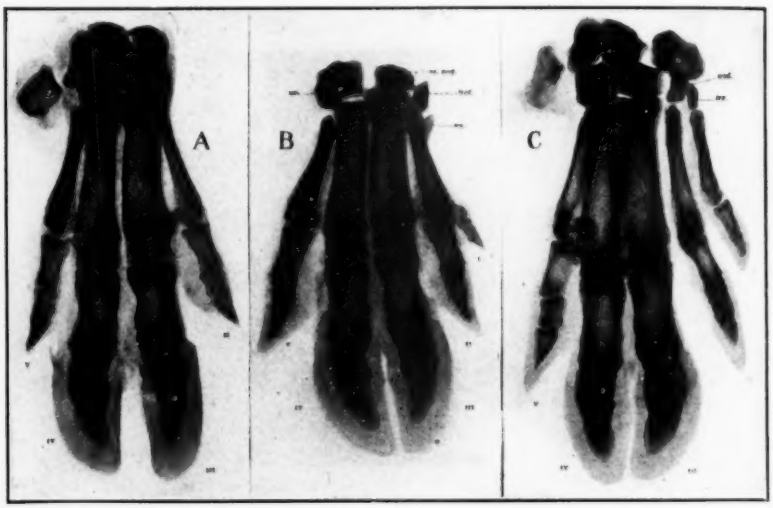


FIG. 4. X-ray photographs of the pig's manus showing normal structure and reversionary polydactylism. *A*, bones of normal manus; *B*, manus in which the pollex is represented by two phalanges and the distal end of the metacarpal bone (I.); *C*, manus with pollex completely developed; *trz.*, trapezium.

(Fig. 3, *A*). It is, therefore, an interesting fact that in the polydactyl swine observed by the writer the extra digits were in every case located upon the manus, and in most instances were undoubtedly developed from the rudiment of the pollex; for the extra digit was attached to the carpal bone as a normal pollex would be, and careful dissections of muscles and nerves gave no evidence of duplications. This does not support Gegenbaur's assertion that the extra digits of swine were developed by the splitting of the second toe. His conclusion was based on the dissection of two 'pig's knuckles' cut off below the carpus. Consequently he could not tell how the extra digit was attached. In any case this was scanty evidence on which to base a general conclusion. The writer was fortunate enough to obtain for study thirty-six perfect specimens. In one type observed, a small hoof, two phalanges and the

distal end of the metacarpal bone were developed (Fig. 4, *B*) and in several cases a perfectly formed pollex was present (Fig. 4, *C*). In its general structure the manus of such polydactyl pigs resembles closely that of the fossil swine *Ancodus,* as may be seen by comparing *A* and *B*

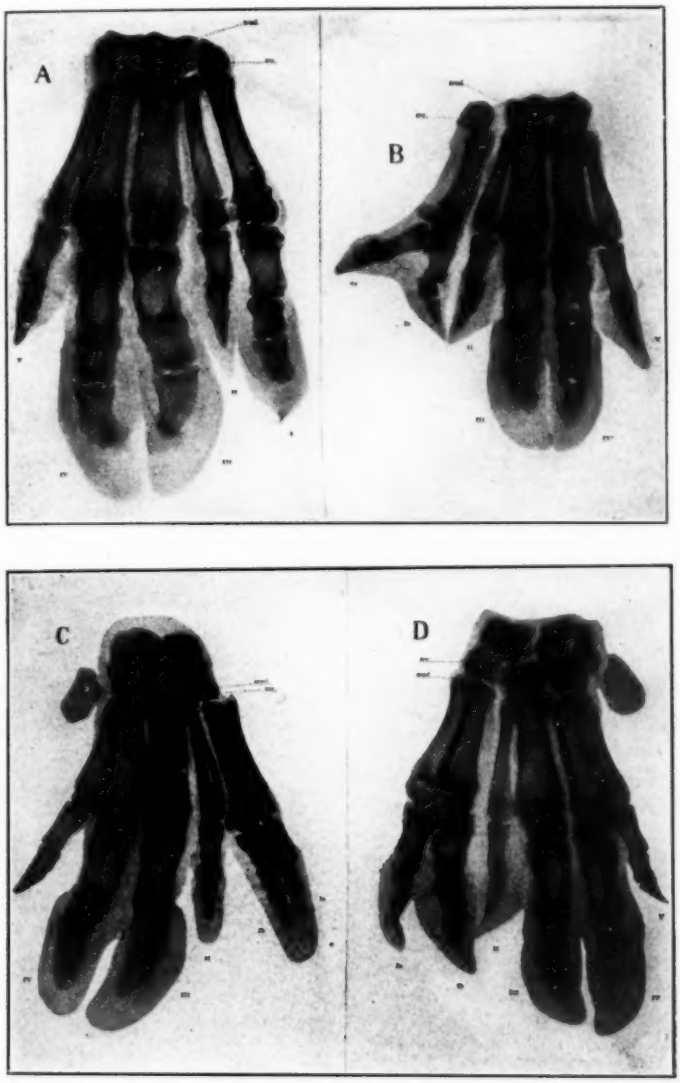


FIG. 5. A series of four X-ray photographs showing variations and duplications of the pollex (I.) in the manus of the pig. *A*, a manus in which the pollex is represented by an abnormally large digit of three phalanges; *B*, the phalanges of the extra digit are duplicated; *C*, all the bones of the extra digit are duplicated, but both sets of phalanges are enclosed within a single hoof; *D*, two extra digits are present, articulating with a single trapezium (*trz.*).

of Fig. 3. In other instances not a pollex, but a digit of three phalanges, was produced, and these in turn exhibited all stages of duplication up to the formation of two large extra toes. But in each case

the two extra toes were developed by the variation of the rudimentary pollex (Fig. 5, *A–D*).

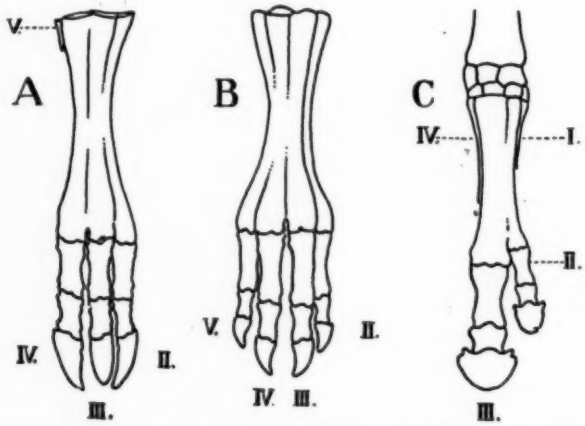


FIG. 6. *A*, calf's manus with digit II. fully developed (from X-ray photograph); *B*, manus of sheep with two extra digits (II. and V.) present (after Chauveau); *C*, manus of horse with two extra digits (I. and II.) (after Marsh).

The writer has also observed two cases in which the second digit of the ox was developed into a functional toe (Fig. 6, *A*), and in the foot of the sheep four complete digits sometimes occur (Fig. 6, *B*). As far back as Roman times the horse is known to have possessed extra toes. Suetonius alludes to a horse given to Julius Cæsar 'which had feet that were almost human, the hoofs being cleft like toes.' Two cases were described by Winter in 1703, and Marsh has since observed the development of an extra digit from one of the splint bones (Fig. 6, *C*); four or five digits may sometimes occur, but all of these are not completely developed.

It is thus clear that the vestiges regarded as digital rudiments are really such, and that mammals possessing these vestiges must at one time have had a greater number of functional toes, some of which later became useless. It is a well-known theory that this reduction in the number of digits was in adaptation to some special function like that of locomotion. It has been carried to the extreme in the foot of the hoofed mammals; and of living forms, the swine and ruminants afford a beautiful series of digital reductions (Fig. 7, *A–H*). Even among living carnivora, forms like the cat and dog have the pollex reduced and the hallux absent, and, as we have seen, the forerunners of the swine had a reduced pollex on the manus (Fig. 7, *A*), and only four digits on the pes. The first digit is vestigial among the hippopotami; the second and fifth are slightly smaller than the third and fourth (Fig. 7, *B*). The difference in the size of the two pairs of digits is more marked in another fossil pig, but the small outer digits still articulate firmly at the wrists and ankle joints (Fig. 7, *C*). The third and fourth toes of the swine are relatively much larger and have taken

unto themselves the whole articular surface of the carpus and tarsus (*D*).

Like the pig, the little water-deer (*Dorcatherium*) possesses four distinct functional toes, but in *Tragulus,* a closely related form, the outer toes are exceedingly slender and do not articulate proximally (*E*). The upper ends of these small digits have been reduced in the foot of the roebuck (*Capreolus carea*); in the extremities of the red deer (*Cervus elaphus*), these digits are represented only by the bones of the phalanges and vestiges of the metacarpals and metatarsals (*F*). In the foot of the sheep the outer digits are reduced to two small phalanges (*G*); these are absent in the foot of the ox and the antelope. Finally, the small hoofs, the only vestiges of the second and fifth digits of the ox, disappear in the extremities of the giraffe and the camel (*H*).

This series of extremities thus shows a reduction from five to two digits. The gradual atrophy of these three toes has been ascribed to the specialization of the foot as an organ of rapid locomotion. Primi-

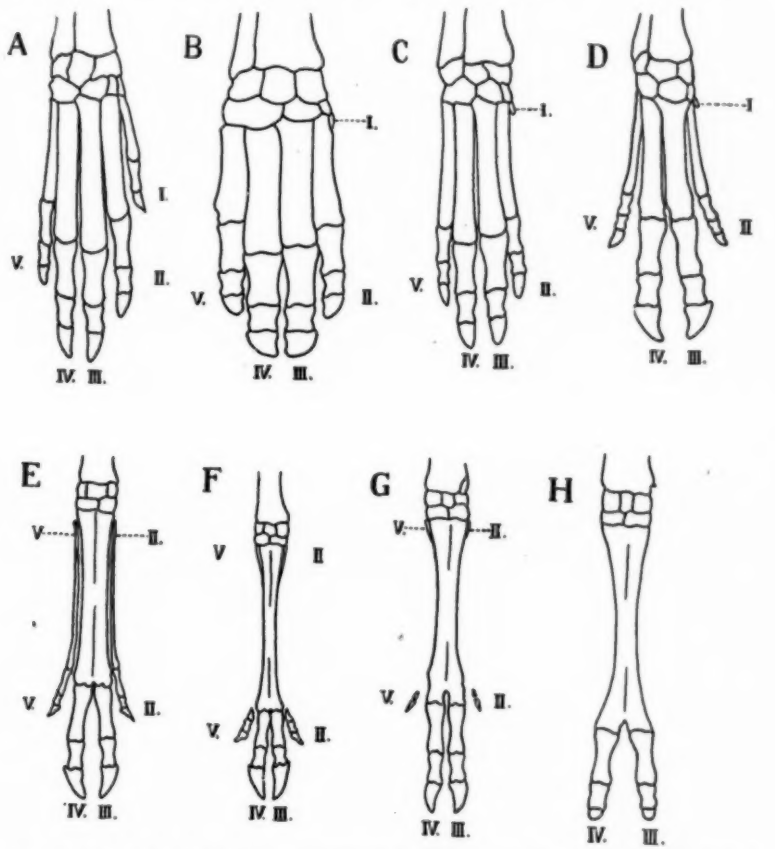


FIG. 7. A series of arterio-dactyl extremities showing successive reduction of the digits from five to two. *A, Ancodus* (fossil); *B, Hippopotamus; C, Hyopotamus* (fossil); *D, Sus; E, Tragulus; F, Cervus; G, Ovis; H, Camelus.*

tive mammals were plantigrades, resting the whole surface of the foot upon the ground in running. This posture is not favorable for rapid locomotion, as instanced by the lumbering gait of the bear. It has been retained only by animals which, through burrowing, swimming, climbing or means other than speed are enabled to escape their enemies or obtain their food. But both to beasts of prey and to their quarry increased speed and leaping power would be of great advantage in the struggle for existence. To obtain this advantage they had recourse to the same expedient to which on occasion plantigrade man still resorts —they ran upon their toes. If by variation, the digitigrade position became gradually, or suddenly, the fixed posture of the foot in progression, the structure of the digits would soon be affected. Provided that the feet were used only in locomotion, the shorter digits would not reach the ground. Being useless, they might soon disappear. The reduction of the digits has, therefore, been ascribed simply to the adoption of the digitigrade posture. This is, indeed, the chief, but it is not the only factor. It does not explain why the hallux of the dog and cat has atrophied, while the pollex persists; why the pig and water-deer have four digits, the giraffe and camel only two, though all are digitigrade.

There are evidently three factors upon which the degree of digital reduction depends: (1) the specialization of the extremity for locomotion; (2) the degree of perfection to which the digitigrade posture is carried; (3) the character of the ground which the animals traverse.

The hallux of the dog and cat has been reduced, because the pes is used only for progression and in the digitigrade position. The pollex of these animals has been retained, as the claw is useful to the cats in climbing and in catching their prey—to the dogs and wolves in burrowing and in holding their prey. It is interesting to note that the pollex is no longer a functional organ in the manus of the hyena, an animal which feeds chiefly on carrion.

To the Carnivora, which are beasts of prey, a padded foot and sharp claws are necessary structures. We, therefore, find that the digitigrade posture is not developed to the extreme. These animals run upon the ball of the foot; whatever may be the character of the country traversed, all four toes are used in progression, and no further reductions have taken place.

To the herbivorous ungulates claws are useless structures, and in escaping from their foes noise is no drawback. Speed is their chief requirement and this is increased by leaping from the tips of the toes. This method of progression would blunt the claws, which would then be modified to protect the toes.

If the digitigrade position is not well developed (as is the case with the slow, heavy ungulates like the elephant, tapir and rhinoceros) all the toes, or all but the first, may reach the ground and function in

locomotion. But if the digitigrade posture be extreme, the fate of the digits will depend upon the habitat of the animal. Should the region ordinarily traversed offer smooth, firm footing, an animal running upon the tips of its toes would use chiefly the longer third digit; this digit would naturally be strengthened and increased in size, while the other digits, being useless, might gradually disappear. Such changes have taken place in the foot of the horse, and in this way the perissodactyl, or odd-toed, type of foot has arisen. As is well known, the habitat of the horse family is the dry rolling plain, and what evidence we have goes to show that the remote ancestors of the horse ranged a similar country. The fact that the digits of the horse were first reduced at their distal extremities points to the same conclusion. For, as we shall see, digits which do not support the weight of the body, may, if the animal frequents swampy regions, keep the foot from sinking too deeply. In such cases the reduction of the digits begins always at the upper or proximal end, just the reverse of conditions in the foot of the horse. We may conclude then that the odd-toed foot resulted from digitigrade locomotion over firm, comparatively level ground.

For rapid progression over swampy ground, the structure of the foot must conform to two requirements. It must be prevented from sinking too deeply and must be easily withdrawn. Any one who has attempted to walk across a mud flat can appreciate the importance of these two factors. It is in adaptation to these requirements that the artiodactyl, or even-toed, type of foot has evidently been developed. If an ungulate was of semi-aquatic habits, or attached to swampy places, all four digits, by spreading, would prevent the sinking of the foot; its withdrawal would be facilitated by making the foot occupy as little space as possible, and this could be accomplished by shifting the proximal articulations of the outer digits inward and posterior to the middle digits. The toes would then be arranged in pairs, the outer pair lying somewhat behind the other. Now in a semi-aquatic animal each pair of digits would be subjected to the same usage in walking or running through boggy ground; the digits of each pair would tend toward the same structure on this account. But as the middle digits would support the greater part of the strain brought to bear upon the foot, this pair of digits would naturally become larger and stronger than the other. Should our hypothetical ungulate change its habitat from the swamp to the firmer footing of the plain or upland, the outer pair of digits would not reach the ground, and unless they proved of use to the animal in some other way we should expect them to speedily disappear.

This theory as to the origin of the artiodactyl foot is supported when we examine into the habits of the even-toed ungulates. The more primitive forms are attached to the water. The amphibious hippo-

potamus possesses, as we should expect, four digits, arranged in pairs of equal size. The wild swine, although attached to water and to boggy ground, are nevertheless swift runners, and spend most of their time on more solid footing. The outer digits are retained because they are useful in keeping the foot from sinking in the mud, though they are not functional a good share of the time. The greater strains brought to bear upon the middle digits have resulted in their increased size (by variation) and their monopoly of the carpal and tarsal joints, firm articulation with which is no longer needed by the little-used outer toes. That these changes were advantageous to the swine is shown by the fact that related forms, less adaptive in this respect, have become extinct.

Of the deer family, the water chevrotain and its relatives are the only forms possessing four complete digits. Again we have to do with animals attached to swampy places, and the outer digits, though slender, are retained intact because of the extra support they offer in traversing boggy ground. The fusion of the metacarpal and metatarsal bones of the middle digits, which characterizes the foot of other ruminants, has evidently been prevented in the water deer by the spreading of the toes.

The red deer is one of the swiftest of runners and its usual habitat is the wooded plain and upland. It, however, readily takes to the water, as a large part of its food consists of aquatic plants. In roaming the more solid floor of the forest only the middle digits support the body. These have become relatively larger than those of swine, and are further strengthened by the union of the metacarpal and metatarsal bones. The outer digits are perfectly useless in ordinary locomotion, but still perform two important functions: they serve to support the foot in yielding ground and give the deer a firm footing when running rapidly, especially down-hill. Any one may observe that in walking on fairly firm ground the foot of the deer leaves but two hoofprints, but that the foot of a running deer leaves four distinct marks. The performance of these functions has caused the retention of the lower portion of the outer toes. But as these digits no longer support any part of the weight of the animal, no proximal articulation is necessary and we find that the upper part of the metacarpal and metatarsal bones has atrophied.

Wild goats and sheep are mountain animals, feeding on rugged and precipitous slopes where the footing is precarious. Both sheep and goats are expert climbers and leapers, but in their ordinary habitat only the middle digits are used for supporting the weight of the body. The outer digits have, therefore, been reduced, but the hoofs and rudiments of two phalanges have been retained, because these small toes are used in climbing, and render the animals more sure-footed. No such function is performed by the second and fifth digits of the

antelope, bison and ox; these animals roam the open plain and upland, and the outer toes would be of no use except for the occasional support of the foot when these animals enter the water to drink. We find, therefore, that only the small hoofs of the reduced toes persist. In the foot of the camel and giraffe even these vestiges have disappeared, as their habitat has long been the dry, sandy plain.

From these observations it seems plain that among ungulates the functions of the digits have been affected by the habitat of the various animals; and that there is a direct relation between the degree of digital reduction and the character of the country traversed. The use of the foot as an organ of locomotion alone, and the assumption of the digitigrade posture, were the primary factors producing reductions of the toes; the degree of such reductions and the type of foot produced have been dependent upon the habitat of the animals. The artiodactyl foot was formed in adaptation to semi-aquatic habits, and as the animals changed their habitat to 'terra firma' a further reduction of the digits resulted. The perissodactyl, or odd-toed, type of foot, began with the assumption of the digitigrade posture by animals which traversed solid ground, and the digits were further reduced as the digitigrade posture was developed to perfection.

It has been assumed that those digits which were useless would disappear. There is evidence that if they were not reduced they would be not only useless, but of distinct disadvantage to the animals. The writer has observed that the extra hallux rarely occurs on the pes of hunting dogs; when it does occur it is frequently injured and sometimes completely torn away. It is also noteworthy that the extra toe is most often found on the pes of the St. Bernard and Newfoundland, in which breeds it may be of some use for swimming, and walking through deep snows. It has been observed, too, that the small hoofs of the sheep and deer grow rapidly on the second and fifth digits, but are normally worn away by daily use. If these animals are kept in unnatural surroundings, as when sheep are deprived of rocky pasture, or deer kept in zoological gardens, the hoofs of the reduced digits will grow long, curved and twisted to such an extent as seriously to impede locomotion. We can readily see that should the wild deer or sheep change its habitat to the smooth footing of the open plains, the same abnormal growths might occur and hinder rapid locomotion. Variations tending toward the reduction of these digits would favor the survival of their possessors, and give rise to the type of foot found among the antelopes, cattle and giraffes.

In conclusion it may be of interest to speak briefly of the digital reductions which have taken place in the foot of the running birds (Ratitæ). The most primitive of the birds exhibited the digitigrade posture, but walked upon the ball of the foot. This may have caused the reduction of the fifth digit (which early disappeared), and cer-

tainly had to do with the reduction of the hallux. The hallux has, however, been retained by most flying birds because it is used in perching, in prehension and in swimming. As the legs of birds are set well forward on the body, they are more widely separated above than below. This position has thrown the greatest strains upon the outer or fourth digit, which is always longer than the second toe. Now in the foot of running birds like the emu, the digits are used only in locomotion and the hallux has disappeared, as it was useless in progression. But the digitigrade posture of the emu is the same as that of the flying birds; the ball of the foot touches the ground and the remaining three digits, being all functional, are well developed. The ostrich, however, has increased its swiftness by running (and walking) upon the tips of its toes. This posture would throw the weight of the body and the work of locomotion upon the longer third and fourth digits. As the foot of the ostrich is used only in locomotion, and as the birds traverse the smooth floor of the desert, the shorter second digit would fail to reach the ground and eventually disappear. As a result we find that the ostrich has only two functional digits.

The digits of birds, therefore, show structural changes which are exactly paralleled by those exhibited by various ungulates, and the digital reductions which have taken place may be attributed to the same factors in each case.

A SANITARY OUTLOOK[1]

By Sir JAMES CRICHTON-BROWNE, M.D., LL.D., F.R.S.

ONE of the hopeful signs of the times is the popular interest that is manifested in health questions. No doubt, as Carlyle said, all men are born hypochondriac, and in all ages—never more so than in the present one—swindlers like Caliogstro have driven a thriving trade in well-advertised potions and specifics, but never before has health in the aggregate been the object of public concernment as it now is; never before have the scientific principles that underline its preservation and the practical methods by which these may be applied become, to the same extent as now, part of the civil polity of the nation. The whole country is valetudinarian now, in the best sense of the word, conscious of its weakness, determined to recover its strength. Topics that not long ago would have been thought suitable only for a medical society are discussed in the streets and across the dinner table, while the newspapers teem with articles on physical deterioration, infantile mortality, tuberculosis and cancer research.

And this is, I think, as it should be. The intelligent cooperation of all classes is needed in carrying on the great work of sanitary reform. There should be no squeamish affectation in ignoring subjects that are of vital and universal significance. There is no mystery in physiology and hygiene, and the better these are understood the greater will be the deference paid to expert opinion in matters in which special knowledge is involved, the clearer will be the appreciation of the boundary where prophylaxis terminates and medical diagnosis and treatment begin.

But the inevitability with which all statements bearing on public health are in these days bruited abroad and the avidity with which they are received make it incumbent more than ever on those who speak with authority on such subjects to observe caution and discretion, for doubts or speculations that would be harmless or even stimulating when addressed to a critical and well-informed audience, may become confusing or misleading when, having passed through the alembic of the journalistic mind, they appeal to the general. I had that brought home to me somewhat forcibly on a recent occasion on reading the newspaper reports, just for one day, of the meeting of the British Medical Association at Leicester. I found there an eminent medical authority reported as giving some countenance to telepathy, which I

[1] A paper read before the second London conference of the Sanitary Inspectors' Association.

am sure ninety-nine hundredths of the medical profession regard as an unproven and in its present shape improbable hypothesis, and throwing cold water on the sanatorial treatment of consumption which, I believe the great mass of the medical profession regard as a valuable addition to our means of contending with that malady. Dr. Maudsley deplored the want of sobriety in some medical statements on the popular platform, in consequence of which the public has jumped to the conclusion that because the bacillus has been discovered phthisis is curable, the old notions of its heredity erroneous, the objection to phthisical marriages obsolete, and the right thing to do forthwith to dot the land with sanatoriums for which, he concludes, not more can be said than for sensible treatment before their invention.

Now, I have made and listened to a good many medical statements on popular platforms respecting tuberculosis, but I have never become conscious of the insobriety which has shocked Dr. Maudsley. On every occasion, three factors in the etiology of phthisis—the seed, the soil and the surroundings—have been fully recognized, and while emphasis has been properly laid upon the seed as the primary and essential cause of the disease, due weight has been given to the greater or less resistance of the living tissues in which the seed is sown, and to the more or less favorable nature of the environment during its germination and growth. Dr. Maudsley is the apostle of heredity and of temperament —matters of great moment—but I do not know of any hereditary predisposition or temperamental condition that will make a man proof against a sufficient dose of arsenic or strychnia, and we have no evidence that there is any that will make him immune to a sufficient dose of the tubercle bacillus of sufficient virulence introduced into his system. The resistance, to the implantation of the bacillus and to its spread and propagation, varies greatly. In some habits of body it will scarce take root; in others it springs up rapidly and flourishes luxuriantly, but congeniality of the soil is a very different thing from hereditary transmission, and there is no kind of inherited constitution or temperament in which in the absence of the seed tuberculosis can be developed. The bacillus has its heredity, as well as its animal or human victim, and it is possible that the occasional failure of its attacks may be due, not so much to the stoutness of the resistance offered, as to the feebleness of the assistants, the descendants of an attenuated stock.

Dr. Maudsley says, 'no one thinking clearly ever thought that actual tubercle may be inherited,' but in saying so he must, for a moment, have lost his wonted lucidity of thought, for Professor Bang has demonstrated that the tubercle bacillus has been found in the livers of the new-born calves of tubercular cows. This mode of transmission of the disease is, however, so rare that it may be ignored, and as it is certain

that tuberculosis is not handed down as gout and insanity are known to be, it is right that the public should be taught that the old notion of its heredity is erroneous, and that the main thing to be held in view is the avoidance of the pestiferous bacillus. But it is right also that they should be taught—and they have been so taught from all platforms of which I know anything—that the marriage of a person actually laboring under consumption or of two persons belonging to families in which a marked liability to take on consumption has been decisively manifested are imprudent and to be condemned.

The belief for which Dr. Maudsley makes our intemperate platform orators responsible, that phthisis is curable because the tubercle bacillus has been discovered must have been promulgated, if it exists, by persons in a state of complete obfuscation; for every medical tyro knows that phthisis was curable and was cured in many cases long before Koch's enlightening revelation, and that the espial of the bane, did not at once guide us to an efficacious antidote. But surely Dr. Maudsley will not deny that the discovery of the one true cause of the disease puts us in an infinitely better position for circumscribing its ravages, for preventing it, ay, and for curing it than we were before. We know now that it is the outgrowth not of any subtle tendency passed on from generation to generation, but of a fungus which invades the body from without, by certain channels and has a definite life history; which outside the body has certain favorite haunts and may be destroyed by certain agents and inside the body may have its growth encouraged or retarded by certain conditions, which it is in our power to create or modify. Phthisis is still killing upwards of 40,000 persons in England and Wales annually. Tuberculosis in all its forms is killing upwards of 57,000, but the mortality from phthisis and tuberculosis has fallen enormously and is still falling. Twenty years ago—and during that time there can be no question as to improved diagnosis or change in nomenclature vitiating statistical returns—phthisis caused upwards of 49,000 deaths; to-day it is causing only about 40,000 per annum; tuberculosis caused upwards of 20,000 deaths; to-day it is causing not more than 17,000. Twenty years ago the annual death rate from phthisis was 1,827 per million, against 1,203 in 1903; the death rate from other forms of tuberculosis was 567 per million, living against 459 in 1903. In as short a period as twenty years the death rate from phthisis was reduced as much as 25 per cent. Surely these figures justify platform speakers in some degree of exultation if not of insobriety, and warrant them in exhorting the people to persevere in the use of the means which have secured such splendid results, and to supplement these by other means suggested by our new knowledge of the cause of the disease. The reduction in the mortality from phthisis and tuberculosis has been due, we know, to subsoil drainage, and the

other great sanitary improvements that have been effected in the last half century, and has taken place in the absence of any special precautions against the dissemination of the seed of the disease. Is it too much to hope that, now that we know this seed and can intercept and destroy it at the shoots by which it is discharged from its culture beds and granaries to be scattered broadcast, we shall be able still further, and more materially to reduce the tuberculosis death rate and the prevalence of the disease? Nay, further, is it too much to hope that by removing those who have contracted the disease from the impoverished, insalubrious and ill-regulated conditions of life that have invited and fostered it and by immersing them in pure air and unpolluted sunlight in restful and hopeful circumstances, with a liberal and well-adjusted diet and under constant skilled medical supervision, so that untoward symptoms are dealt with as they arise and every bodily function is ordered, as far as may be, in the interests of health—and this is what sanatorium treatment consists in—is it too much to hope that we shall thus save many lives that would otherwise be lost, and prolong the days and alleviate the sufferings of those who are beyond hope of permanent recovery? Our sanatoriums in this country have not yet been in existence for a sufficient length of time to allow of the collection of wholly trustworthy statistics, but the returns as far as they go are highly encouraging, and confirmatory of the favorable verdict on sanatorial treatment arrived at by German institutions. Dr. Maudsley, himself, admits that so far the outcome of experience seems to be that many patients who are sent to sanatoriums in the early stage of the disease, recover if they are kept long enough, that most of those in a more advanced stage improve while they are there, frequently relapsing afterwards, and that those who are badly diseased ought not to be sent at all. And this, he calls a modest result. I am disposed to describe it as a result of which we may well feel proud and as one that, if properly presented to the public, should lead to the adoption on a larger scale than hitherto of this system of treatment at that stage of the disease when it may prove so efficacious. The benefits to be derived from sanatorial treatment have perhaps been exaggerated in prospect. It can not altogether supersede other forms of treatment, at high altitudes on sunny littorals, on the veldt, prairie or desert, or by sea voyages; it can not reconstruct a disorganized lung, but to those whose means do not enable them to command the best treatment under private care, and in whom the tubercular lesions are still of limited extent, and leave enough breathing space, it opens up new hopes of restoration to health. Even to the affluent, sanatorial treatment is profitable in the medical discipline it involves. The time may come when science will give us some tuberculin, or serum, or antitoxin, or antiseptic, that will kill the tubercle bacillus in its hidden lair, counter-

act its poisonous products, or reinforce the phagocytes in their attacks on it, but meanwhile sanatorial treatment gives expectations of recovery greater than those of any other kind of treatment that is known to us, and it seems to me inexpedient to say anything which may discourage the benevolent from putting it within reach of the poor and needy, or hinder the poor and needy, stricken with tuberculosis, from taking advantage of it. Even if sanatorial treatment were not superior to home treatment in the number of cures it effected, it is still deserving of support because it withdraws, for a time, from their own homes and from places of public resort persons who are jets of deadly dust, and thus diminishes the diffusion of tuberculous disease. And surely even the arrest of the disease, which Dr. Maudsley admits is secured by sanatorial treatment in advanced cases, is worth having. Even a damaged life is sometimes sweet to its possessor and precious to those who hold it dear; and it will be a sad day for humanity when the prolongation of life under all circumstances ceases to be the chief aim of the medical profession, and when euthanasia procured or suffered, is recognized as a justifiable mode of exit from the sick room. But beyond all this, even in hopeless cases, in which no arrest is secured, sanatorial treatment is not without its merits, for all patients who have undergone it return to their homes educated in the procedure that is necessary to make them innocuous to others, and trained how to deal with their infectious expectoration, and thus again the propagation of the disease may be in some measure limited.

But Dr. Maudsley is not only sceptical about sanatorial treatment, but apparently doubtful of the wisdom of any sort of curative treatment in tuberculosis. The ordained function of the bacillus in the universe is, he suggests, to make away with weak humanity. The loss to the community by the death of consumptives is not, he hints, as real as is imagined. "Might not the ultimate cost to the commonwealth," he asks, "be greater, were those persons allowed to go on living and breeding in it." The assumptions here are that consumptives inevitably breed consumptives, and that the tubercle bacillus invariably fastens on weak humanity, and both these assumptions are erroneous. Recent inquiries have shown that the influence of heredity in consumption is not so great as was at one time believed. Dr. Claud Muirhead found, after an elaborate investigation, and with peculiar facilities for arriving at the facts, that out of five hundred and twenty-four cases of death from phthisis, only one hundred and twenty, or 22.89 per cent., presented in their family history distinct evidence of direct phthisical taint, and other 62, or an additional 11.83 per cent., exhibited a suspicious family history of phthisis. That is to say, at the very outside, only 34.72 per cent. of these five hundred and twenty-four persons who died of consumption, exhibited in their family history any evidence of

family predisposition to the disease. This percentage accords fairly closely with the published statistics of Dr. Williams and Dr. Cotton, who give, as the result of their investigations into this point, 34 per cent. and 36 per cent., respectively. In an inquiry carried out by Dr. Squire he found that while about 33 per cent. of consumptives present a family history of tuberculosis, statistics give grounds for attributing the disease to occupations and surroundings in by far the greater number of these cases, and place the possible influence of heredity at about 9 per cent. instead of 33 per cent.

It is certain that persons who have recovered from consumption breed perfectly strong and vigorous children, who remain throughout life free from the disease, and it is preposterous to suggest that if we succeeded in saving the lives of the 40,000 persons who die annually of consumption we should have thereby added to the burdens of the community. We should thereby directly and indirectly have secured enormous economic advantages in the productive industry of the persons saved, and in their contributions to the maintenance of those dependent on them. Mr. Baldwin Latham estimates the saving to this country in twenty years, by sanitary work, in funerals avoided, sickness prevented, and wage-earning powers retained, at £267,141,060; and of that huge sum a big slice must go to the credit of tuberculosis.

Then again, vulnerability to consumption does not necessarily imply either bodily or mental weakness. The disease is most fatal in the prime of life, and strikes down, not merely the feeble and incapable, but the strong and vigorous, catching them at some moment of temporary debility. The intellectually gifted seems to be peculiarly susceptible to it, and it has robbed the world of incalculable benefits in the fruits of genius. It is not by any means merely an eliminator of waste material, but a ruthless destroyer of some of the finest elements of our species, and we need have no misgivings in resisting it and in doing our best to extirpate it altogether. The enormous reduction that has taken place in the mortality from consumption has been an unmixed good, and its final disappearance from amongst us, which is not a chimera, but a reasonable anticipation, will be attended by nothing but gain to mankind.

Dr. Maudsley thinks we shall never be able to keep bacilli out of the body. Well, as regards the tubercle bacilli, we mean to try! And his gloomy prognostications in this matter are considerably discounted when we find associated with them some disparagement of antiseptic surgery and of the sterilization of food because, forsooth, there are hundreds of different kinds of bacilli in the human mouth and intestines, and because the nutritive value of certain kinds of food may be reduced by sterilization. Our operating theaters, as they exist to-day, and every kitchen range, are a standing protest against Dr. Maudsley's

extraordinary impeachment. Surgeons do somehow succeed in excluding from wounds bacilli of an injurious character in injurious numbers, and a recent experience in Birmingham suggests that the ice-creams there would have been none the worse for sterilization by boiling, even at the sacrifice of the whole of their nutritive and glacial virtues.

I venture to think that Dr. Maudsley has spoken too despondently about the sanatorial treatment of consumption, and I regret the wide publication of his views, because, coming as they do from one so eminent in his profession, they may tend to check a movement of great promise.

In the same newspaper that contained Dr. Maudsley's fling at sanatoriums, I read a report of a discussion on physical deterioration that must, I think, have proved somewhat bewildering to the man in the railway train. Physical deterioration was affirmed and denied; it was traced to education and to the want of education. It was declared to be decimating our infant population and to be non-existent till the age of thirteen. It was ascribed to underfeeding and overfeeding, to cheap sweets and cigarettes, to maternal neglect, paternal drunkenness, and the want of a Minister of Public Health of cabinet rank. I can not pause to reconcile these apparently divergent views, for, of course, they are reconcilable, but there was one statement made so startling that I should like to refer to it more particularly. And that was that 'environment would knock heredity into a cocked hat,' a statement leading to an article in the paper headed 'The Bubble of Heredity Pricked,' which must mean that organic creation has burst up. Now it may be well that there should be a reaction against an extreme and fatalistic belief in the power of ancestral sour grapes to set the children's teeth on edge, but we can not altogether dispense with heredity, and any one who will contemplate a sheep and a cow and a goose and a rabbit, all brought up on the same common, fed on the same grass, and exposed to the same weather, will realize that there are limits to the power of environment. Tremendous are the potentialities pent up in those little particles of protoplasm—the germ and sperm cell. The truth is that heredity lies at the core of things, while environment plays on the surface. Their reciprocal influences may be detected in every living being. Heredity modifies environment, and environment deflects heredity, always within bounds and under some higher authority that controls the two. The plan of the edifice is practically fixed, but its dimensions, stability, symmetry, soundness and adornment, are subject to modification as the building goes on, and must depend largely on the nature of the material supplied and on the character of the builders. Heredity is, in every individual, made up of two convergent hereditary streams, and becomes solid at the center, but has a fluent edge, and it is on that that environment operates. It is of great im-

portance that we should accurately distinguish between these environmental influences that are temporary in their effects and modify the individual or existing generation, and those that are permanent, and, as it were, sink in and modify the race.

It was in connection with the former of these that the contemptuous treatment of heredity at Leicester, to which I have alluded, took place. Dr. William Hall, who has done so much to stir up an active interest in the feeding of school children, impressed by the prompt and striking results he had witnessed by beneficially influencing their food environment, threw discredit on heredity, and not only so, but argued that there is really only one important element in environment, and that is food. He went so far as to say that food altered the whole condition of the individual, and that the children in the slums of our great cities, properly fed, could be reared superior in physique to children reared in better class districts, which, from his own point of view, proved rather too much, for if the slum children when well fed are superior to the better class children, presumably equally well fed, then they must have inherited more vigorous constitutions, or the better class children must be retarded in their development by conditions other than food. Amongst the Jewish children in Leeds, examined by Dr. Hall, who were so much stronger and less rickety than the Gentile children living in the same district, careful feeding may have been, and probably was, the principal factor in their better health and vigor, but there were other factors which should not be ignored. Racial characteristics must count for something. Dr. Hall says that the poor Jew is more self-reliant, temperate, and has a greater power of resisting infectious disease than the poor Gentile. Does he suggest that these traits must also be attributed to feeding? Then the Mosaic law bears on personal hygiene through other channels than that of diet. The Tenth Ward in New York, the population of which consists almost entirely of Russian and Polish Jews, is the most densely populated in the city, both as regards the number of inhabitants to the acre and of tenants to the house, and notwithstanding this the Tenth Ward has the extremely low death rate, for New York, of 17.14, and is surpassed in healthfulness only by two wards out of the twenty-four of the city—one a business, and the other a suburban district. Now this favorable death-rate and general salubrity of the Tenth Ward are not the result of superior economic conditions, or better feeding, for the people are of the very poorest class, but must be credited to cleanliness and that careful observance of domestic sanitation in all its branches, enjoined by Hebraic rule and custom.

No one will underrate the importance of the part played by food in physical development, or the sinister effects of a deficiency of it, especially when growth is going on, in the production of degeneration; but, as Dr. Dawson Williams pointed out, it is going far to say that the

whole of the unfitness of the race is attributable to the lack of food. Many other causes contribute to that. A little later Dr. William Hall seemed himself to realize this, for he affirmed that poverty—a very comprehensive term, covering a multitude of evils—is ultimately responsible for the unsatisfactory physique of our people. Luxury has its degenerates as well as poverty, but poverty is the wholesale degenerator, and it is, therefore, I am sure, with immense satisfaction that all we who are interested in the public health have heard that it is the intention of the government to appoint a Royal Commission to inquire into the working of the poor law. It is to be hoped that the deliberations of that commission will lead not only to the adaptation of the poor law to modern social conditions, but to the discovery of efficient methods of dealing with what may be called incipient pauperism, or pauperism in the making, of distinguishing between professional paupers and the widely different classes that are from time to time in need of relief owing to fluctuating economic conditions, sickness, immaturity, or senile decay, and of ensuring that there shall no longer be death or disease due to actual starvation amongst us. If the commission can solve the problems thus indicated, and if at the same time our statesmen can in their wisdom, by free trade, or retaliation, or tariff reform, or colonial preference, or in any other way, secure steady employment to all who are willing to work, we may then feel sure that the golden age will not be long delayed.

But we can not sit with hands folded waiting for the golden age to be conferred by any government or commission. We must strenuously persevere in our endeavors to ameliorate the condition of the people, and this we can best do by improving their environment in the widest sense. It is with environment you are officially concerned, and sure I am that you have already by your up-hill labors in mending it left your stamp on the condition of the people. And, indeed, I am inclined to think if there had been no sanitary science and no sanitary inspectors, the environment in this country would by this time have been pretty nearly empty in certain localities. The right hand of the medical officers of health, and with special functions of your own, you have in a multiplicity of ways promoted that cleanliness which is not inferior to godliness in giving a man length of days in the land. You have sweetened our lives by curbing the offensive cupidity of tradesmen and manufacturers. You have protected us from secret poisoning in our food, on a scale that the Borgia never dreamt of. You have, at no small risk to yourselves, warded off from us contagious, infectious and epidemic diseases, and extinguished sparks of them, which but for you might have become ruinous conflagrations. You have even in certain cases provided us with mortuaries and superintended our burial.

Your duties as sanitary inspectors bring you into intimate contact

with the people of all classes; you are better acquainted than any one else with their environmental conditions, and you will, I think, agree with me that of these the one most urgently in need of consideration at this moment is their housing. It would take many addresses to deal with the housing question in all its aspects. It is a large question. We have, on the one hand, men with half a dozen houses of palatial size, standing in broad demesnes, empty for the most part or thinly populated by a retinue of pampered domestics, and we have, on the other hand, half a dozen pinched families huddled into one mean hovel reeking with filthy effluvium. It is, of course, mainly with the hovel-dwellers that sanitary reformers are concerned, and these present difficulties which may well tax their energies for a long time to come. They are everywhere, for from all parts of the country come complaints of over-crowding in wretched dwellings. It is, of course, in the large towns where benevolent enterprise is moving that we hear most of these evils; but they are by no means confined to the great centers of population, in which, however, they are growing at a rate that can no longer be overlooked. Our town population is, as you know, swelling portentously at the expense of the country. Thirty years ago the population of England and Wales was equally divided between town and country, but now three fourths of it are town dwellers, while only one fourth remains on the land, and the cry of the town is 'still they come.' According to the last census, the persons enumerated in urban were to those in rural districts as 335 to 100, whereas ten years previously they were as 250 to 100. The increase in the proportion of the population in urban districts is due partly to the growth of these districts themselves through the absorption of areas which were previously rural, but in a far larger degree to the migration to the towns of country people, and, as the provision of housing accommodation in urban districts has by no means kept pace with their increase of population, overcrowding has thickened and slums have multiplied.

I need not describe to you the state of matters which has resulted—a state of matters in many places deplorable and repulsive. We have in London 300,000 persons living in families of two or more in one-roomed tenements in which privacy and decency are impossible, often without the smallest ray of sunshine summer or winter, with walls and floors in every stage of dirt and decay, with an atmosphere that is stifling and not seldom alive with vermin. Mr. Burns told us that not long ago in Glasgow, where the housing problem is being so vigorously grappled with, there were places where the floors of the houses were let out at a penny or twopence a place so that any one could lie down on his pennyworth, and all huddled together for warmth in a dense mass of struggling humanity till the morning came. "There were," he said, "two places where the only accommodation given was a cord

stretched across the room on which, on the payment of a penny, men were entitled to rest their arms and sleep standing." I do not know that in many places things are quite as bad as that, but in all our large towns and in our small towns, too, housing conditions and overcrowding exist that are an outrage on decency and a disgrace to our civilization.

And heavy are the penalties we pay for these housing conditions and this overcrowding in combination with other insanitary influences that appertain to towns! The urban death-rate for England and Wales is 17 per 1,000 living; the rural death-rate is 12.9; the urban infantile death-rate is 165 per 1,000 births, the rural rate is 126. In every city and town with the increasing density of population on square space, there is an increasing general and special mortality at all ages, but particularly under one year, in insanitary areas. Typhoid fever causes a much greater loss of life in the town than in the country. The urban death-rate from pneumonia exceeds the rural by 87 per cent. The mortality from consumption is at the rate of 1,298 per million living in urban districts and of 1,108 in rural districts. Urban areas suffer more severely from cancer than do rural areas. And almost all these diseases, as well as others which I have not mentioned, because they figure less largely as causes of death, are most prevalent in the most densely built parts of the town, and in the most densely populated areas of these parts, and prevail in these areas in proportion to the number of inmates in the houses, of persons per room, and of insanitary dwellings such as back to back houses, stable dwellings, tenement houses, cellar dwellings and flat houses.

That the townsman is shorter lived than the countryman is incontrovertible. Dr. Tatham calculated that in the rural districts of England the average expectation of life at birth is 51.48 years for males and 54.04 for females, whereas in Manchester it is only 28.78 for males and 32.67 for females, which means that each male has to sacrifice 10.48 years or 39 per cent. of his life, and each female 9.82 years or 34 per cent. of her life for the privilege of being born in an urban area. To show the social waste involved in such heavy mortality, it is enough to point out that 100,000 males born in Manchester would be reduced to 62,326, and 100,000 females to 66,325 in five years; while in the healthy districts it would take fifty and forty-eight years respectively to bring about the same reduction. Clearly the concentration of the population produces a prodigious drain on the vitality of the people, another indication of which is supplied by Dr. Shrubshall's observation that town life tends to extinguish the fair-haired Scandinavian and Teutonic elements in our people which are giving way before the brunette elements of southern derivation.

And the pernicious consequences of such concentration are dis-

cernible in other directions. The children reared in towns are on the average at all ages, shorter, lighter and of inferior chest-girth when compared with those brought up in the country. They suffer in a larger degree—and in some towns to a very alarming degree—from rickets, decayed teeth, defects of vision, deafness, adenoids, glandular enlargements and affections of the heart and lungs, and again it is demonstrable that all these degenerative changes are more numerous in children living in houses of one or two rooms than in those living in houses with three or more apartments.

I need not proceed with the sanitary indictment against town life as now constituted. Its misdeeds are written in characters unmistakable to any one with half an eye in the pale faces, and stunted and misshapen bodies seen in swarms in slum areas; and are recorded in family Bibles, if such pious mementoes are still in vogue, for Mr. Cantlie, after prolonged and careful search, could not find a single person whose ancestors, from their grandfathers downwards, had been born and bred in London. But I should like to say a word or two about one of the countervailing advantages of town life, which is often insisted on and that is, that by the mobility and stimulus it affords, it encourages that ascent of individuals from the lower to the upper social ranks upon which the salvation of society depends. It is, we are told, the concentration of population in cities which best promotes the process of bringing capable men to the front, and recruits the real aristocracy of ability and character amongst us. And if that is so then we must be content to put up with a good deal of destruction of human vigor, in return for the work done by cities as instruments of natural selection in weeding out the incapable and inefficient and advancing the more capable members of society, and in providing us with intellectual leaders. But is city life likely to accomplish all this?

Professor Karl Pearson, a very thoughtful and cautious anthropologist, has told us that decadence of character and of intelligent leadership is to be noted alike in the British merchant, the professional man and the workman. There is a paucity, he says, not only of the better intelligence to guide, but of the moderate intelligence to be guided. And this he attributes to the fact that the intellectual classes are not reproducing their numbers as they did fifty or a hundred years ago. And in this view Professor Pearson is supported by the Prime Minister, who said at Cambridge last year, that in the case of every man who left the laboring class, and became a member of the middle or wealthier classes, his progeny were likely to be diminished, owing to the fact that marriages are later in that class. The prospect thus presented to us is, it must be admitted, a lugubrious one. The better we educate our people and the greater the facilities we give to boys and girls of ability in the lower classes to rise in life, just by so much shall

we deteriorate the race intellectually, for physical characters are not manufactured by school or college, but are bred in the bone, and if our intellectual classes are physically enfeebled by their intellectual exertions, are enervated by wealth and the love of pleasure, or restrained by prudence born of a wrong standard of life, so that they fail to supply us with a due proportion of intellectuals, then progressive decadence is in store for us.

For my own part, however, I am inclined to think that intellectual decadence, if it is upon us, is not altogether due to the causes assigned by Professor Pearson and Mr. Balfour, and is not necessarily destined to deepen as time goes on. In a people like ours, there is always outside the actually intellectual class, a still larger class, potentially intellectual with abilities incompletely evolved, because never called forth, but capable under stress of circumstance of the higher development, just as an ordinary working bee is capable of conversion into a queen by appropriate feeding. This potentially intellectual class, more prolific than the actually intellectual, may make up for its deficiencies and, breeding true or with favorable variations, supply us with intellectual leaders as good as any we have hitherto had.

Then I am quite sure that the educational ladders, provided hitherto to enable children of the humbler class to climb up in the social scale, do not by any means ensure the transference of the intellectuals from the lower to the higher level. They are mounted by the nimble, the quick-witted, the precocious, whose intellectual energies are in many instances soon exhausted, and around the foot of these ladders there remain numbers of children of really finer intellectual power but slower of growth than those who have scrambled up them. We have thus in our humbler or uneducated class, as they are called, a reserve of intellectuals of undiminished fertility, capable of supplying recruits to the intellectual class of the next generation. Many of our finest intellectuals have sprung from the unintellectual class, and genius is generally more or less of a sport.

My own view is that any dearth of ability from which we may be suffering or by which we may be threatened is to be ascribed not so much to the infertility of the cultivated classes as to the artificial production of stupidity in various ways and to the incessant draining from the country, which is the fit and proper breeding place and rearing ground of intellect, of the best elements of our people to be swallowed up, and exterminated or deteriorated in our big towns. We keep nipping off the buds of promise, and if we insist on having lots of green gooseberry tart, we must be content to go with less of ripe gooseberry jam. As Dr. Ogle has said, "the combined effect of the higher mortality of the town and of the constant immigration into it of the pick of the rural population, must clearly be a gradual deterioration of the

whole, inasmuch as the more energetic and vigorous members of the community are consumed more rapidly than the rest of the population." "The country community," remarks Professor Ripley, "grows from its own loins; the city community grows almost entirely by immigration." The country community, mentally as well as physically, develops from within. It is conservative, strong, steady, tenacious, and transmits its mental characteristics, little altered, to the next generation. The city community, on the other hand, accretes largely from without. It is progressive, mobile, fickle, of unstable equilibrium, and under the stress of competition, undergoes mental modifications, which (*Pace Weismann*) it passes on to its successors. And the consequences of the increased instability of the city community are patent enough. Insanity and suicide, both essentially characteristic of industrialism, are far more frequent in business centers than in the homes of agriculture. This does not, however, signify that the mental powers are really more active in the one than in the other. The notion, indeed, that the country laborer is duller in intellect than the man of the same class in the town is untenable. "It is a common assumption," says Professor Wright, "that the country-man is of so limited capacity that he makes use of no more than 300 words. What a libel! The number of words in dialects at the most moderate estimate is over one hundred thousand. In Yorkshire alone, I can call to mind 30,000 different words. If we take the whole of the dialects and put them together, as representing the vocabulary of the working class of this country, and exclude from the English dictionary all technical terms and obsolete words, I venture to say that the number of dialect words will far outnumber the words of the dictionary."

And not less untenable than the notion that the agricultural laborer is dull of intellect is the idea that the city urchin is cleverer and better endowed mentally than the little yokel. Some years ago, Mr. Horsfall asked the opinions of the head masters of two large pupil teacher centers on this point. In both centers there were a number of pupil teachers from the schools of a large town and others who had been taught in country schools. "Both the masters said, that though as a rule, the urban young people were at first brighter and quicker, those from the country, in the long run, showed more staying power, and that their knowledge of country things gave them a great advantage over their town comrades." The conclusion of these masters is in complete accord with that which I arrived at a number of years ago. After a comparative examination of some London Board and Scotch Parish Schools, I found the London children much sharper, more vivacious, and, it must be admitted, more attractive in demeanor than the Scotch children, but the latter, although somewhat stolid and awkward, had decidedly more grasp of intellect and more sound knowledge.

The rule seems to be that the mental development of children is hastened by city life, but soon stops short. Up till thirteen or fourteen they are precocious and then come to a standstill. "At its best," says Dr. Stanley Hall, in his work on Adolescence, "metropolitan life is hard on childhood and especially so on pubescents, and children who can not pass those years in the country are robbed of a right of childhood that should be inalienable, and are exposed to many deleterious influences which jeopardize both health and morals."

City life at its best is bad for children, involving as it does early puberty, exciting distraction, superficiality of knowledge, insufficient repose, and the want of the soothing influences that the country affords, and at its worst when it means a tight squeeze in squalid dwellings, poor food, foul air, foul language, contact with vice, and manifold temptations, it is utterly demoralizing. The chief constable of Glasgow who had to report an increase of juvenile crime in that city, notwithstanding the most strenuous efforts of the police to prevent it, informed the Royal Commission on Physical Training that juvenile depravity was regulated to a large extent by the home influence on the child, the period between twelve and fourteen being that when the mind is most susceptible to influence for good or evil. "Amongst the lower class in the city," said Mr. Ross, "of course one finds the children most depraved, the parents or guardians in many cases being criminals of the lowest possible standard. Street trading is undoubtedly a curse to this class of children. It has been proved again and again that the street gamin is second to none in vice and wickedness of every conceivable kind, in fact, he reduces the commission of a crime to a fine art. If, however, he is taken from his evil surroundings and placed in an industrial school or reformatory he, in the majority of cases, turns out a success in life."

The facts and figures I have been quoting represent the city as an instrument of physical, intellectual and moral degradation. They represent it as sucking in the crude vigor and vitality of the country, sophisticating and enfeebling them by its rigorous competition, and ultimately turning them into inefficiency. It seems obvious that if the city goes on growing at the nineteenth-century rate, and under nineteenth-century conditions, it will dry up the reservoirs of strength in the population, and leave an immense proletariat of inferior quality and without commanders.

But the shield we have been examining has another side. Big cities are with us and are likely to remain. They have sprung up in obedience to economic laws, and they contribute to wealth, for production increases with increasing concentration of population, and wealth redounds to the advantage of the whole country. They favor specialization and enable every man to make the best of any talent or skill he

may possess. The markets they open up stimulate improvements in agricultural methods, and the industries and commerce they establish conduce to good government and individual liberty. They are the nurseries of the arts and sciences, and as to the evils attending them, on which I have been enlarging, they are not all inherent in their very nature, but are largely accidental concomitants of their mode of growth and the offspring of the ignorance and stupidity of their inhabitants. Many of these are in process of mitigation and I dare say it occurs to you that if you had a free hand in demolishing and reconstructing one of our great cities with, say, the cost of the South African war at your disposal, you could free it from much of the opprobrium in relation to sanitary matters that has hitherto attached to it. But such wholesale remodeling is scarcely practicable, and even were it accomplished, the city would still probably fall short of the country standard of health, even if that standard remained where it is at present and were not raised like that of the city. For as things now are, the country is in many parts guilty of sanitary offences as heinous as those of the towns, and is only saved from their consequences in an equal ratio by the wider elbow-room it affords, and the fresh air and unpolluted sunshine it enjoys by nature's bounty. The last royal commission that reported on rural housings described the conditions under which many agricultural laborers live as 'physically and morally unwholesome and offensive.' A London association that in 1897 conducted a systematic inquiry into the state of 240 country villages with 10,000 dwellings declared that in one half of these villages the cottages were bad, and that in some thirty villages there were cases of gross overcrowding. Mr. Walter Crotch has stated that the result of his own very extensive and searching investigation had been "the discovery of whole villages without a drop of water from end to end; of cottages without even the ordinary conveniences which the law of common decency demands, and of poor people having their homes let while they lay quivering in the throes of death." "Week by week," he goes on, "the most shocking cases of overcrowding are reported in the newspapers, and there can be no doubt that this huddling together of people of both sexes and of all ages in the same room is a source of frightful immorality."

Mr. Clement Edwards said in 1900 that many of the inhabited cottages in the south and west of England were in a hopelessly dilapidated condition with gaping walls and rotting roofs, and were, moreover, terribly overcrowded. Some of the facts he reported were positively revolting in themselves and much worse in their suggestions of inevitable social and moral results. As to sanitation it was non-existent. Miss Constance Cochrane, of the Sanitary Institute, quotes a case in Cambridgeshire, in which eleven members of one family were all sleeping in one room owing to the scarcity of cottages in the village. Private

enterprise has failed to furnish anything like adequate accommodation for agricultural laborers, and owing to heartless indifference, indolence or official obstruction—perhaps in some degree also to their own complicated ambiguities—acts of parliament, such as the housing of the working classes act of 1890, have remained practically in abeyance. The result of all this is that lamentable abuses—which perhaps the amending act of 1900 may in some degree remedy—still abound on every hand, and that our scattered hamlets, instead of being idyllic abodes of peace, purity and health, have become hot-beds of discontent, dirt and disease.

But in spite of all this, the country, measured by every standard, remains more salubrious than the town, and, as it is certain to participate in those sanitary improvements which in the progress of medical science and of governmental activity in such matters must come, still further to lower the town death-rate and raise its vital energy, it will probably always maintain its position ahead of the town in salubrity. 'The life of the great city,' said Mr. Henry George, 'is not the natural life of man.' He has an affinity for the open fields, and just as the mortality of city adults must always exceed that of rural adults, on account of the more dangerous nature of town occupations, so must the health of a town population, as a whole, be inferior to that of a country population, because of the more unfavorable nature of its topography. The grouping and close proximity of houses interfere with ingress of sunlight and movement of air, and facilitate the spread of zymotic diseases, which often leave permanent debility and defects behind them. The close agglomeration of numbers of human beings, especially in a state of indigence, is conducive to uncleanliness, and to the generation and diffusion of poisonous exhalations of many kinds. And the larger the grouping, and the closer the proximity and the denser the agglomeration, the greater do the risks become, so that in the interests of humanity there should be some limit to town extension and stringent regulation of town organization. Industry says men must aggregate. sanitary science says they should be permitted to do so only so far as is not incompatible with the welfare of the race, and under well-understood safeguards.

We have been contrasting the merits of town and country from a health point of view, and the conclusion must be that while the country is entitled to the preference of the sanitarian, both are urgently in need of his attentions. Excellent and fruitful work has already been done in both, but much remains to be done, and, as I have already said, the most clamant want of the moment is, it appears to me, the application of remedies to relieve the pressure caused by the increase of population in urban centers.

You are acquainted with the remedies which have been proposed

for that state of things, *viz.*, regulations directed against overcrowding; the acquisition of special areas by the authorities for the obligatory rehousing in the same neighborhood of those disturbed under parliamentary powers; and the acquisition by municipalities of vacant land for the construction of suitable dwellings. These are excellent as far as they go, but seem to me to be palliatives rather than remedies. They shift the load a little but do not really lighten it, and it has been, perhaps, the perception of their futility that has been responsible for the half-hearted manner in which they have been applied. Real relief is only to be obtained by establishing an outflow from the center to the circumference, and it is by affording increased facilities of locomotion that this may be done. It is to the new motive power that is now advancing with such giant strides that we must look for the removal of some of our housing embarrassments. Railway extensions, tube railways, surface and subsurface tramways, and motor omnibuses and cycles will inevitably bring into existence a number of new suburbs around our big cities, to which, if the cost of transit be kept low and rents remain modest, many of the poorer classes who are not compelled to live near the factory or shop will resort, all the more readily if a shortening of the working day gives time for the journeys to and fro, and if associations be formed to help them to become the owners of their houses. And to these suburbs, should the cost of transit and the time occupied by it or high rents prove prohibitive to the working classes, the well-to-do will in numbers retreat, making room for their humbler neighbors in the inner circles. It is probable, too, that these new suburbs would in some degree intercept the streams of population that are perpetually flowing into the towns from the country, for statistics show that as regards London, at any rate, immigrants settle mainly in the most outlying parts.

The new suburbs of towns will, of course, always spring up on lines of communication and where facilities are offered for building speculation, and spread out around, but it is to be hoped that they will be taken in hand in time, and means devised to limit their indefinite expansion. Mr. Charles Booth has said that towns advancing, show a noticeable tendency to shoot out tongues like the sun's corona, the intervals between them being filled up later, and it is this filling up of the intervals between them that should, if possible, be prevented. Island-suburbs are well enough, but when they swell out, become continuous, and form a girdle round the parent town, they aggravate its evils, and help to strangle it. It has been proposed that air should be supplied to the center of great cities by mechanical means—by the Shone vacuum system, for example, in connection with tube railways—but infinitely preferable to any such artificial arrangement, necessarily finical and liable to break down, is a liberal scheme of natural ventila-

tion. There should, it seems to me, be maintained, in connection with all great cities, a series of broad avenues converging towards them from all the points of the compass, free from buildings, and covered with vegetation. The parks and open spaces in our cities are called their lungs, but the lungs are not of much use without the windpipe, and the green avenues I suggest would act in that capacity, and allow an inrush of fresh air and the escape of the vitiated air which is always accumulating in cities. These avenues, I have said, should be clothed in vegetation, and to my thinking the preservation of vegetation, not only around our great cities, but throughout the country generally, is becoming a matter of grave import. Sir James Dewar once calculated that a healthy man evolves on the average about 200 pounds of carbon in the form of carbonic acid annually, and as an acre of the best cultivated land fixes annually about 2,200 pounds of carbon, it follows that one acre of land can economize as much carbon as is supplied by eleven persons. The Crystal Palace covers an area of sixteen acres. If the atmosphere had to be kept pure by interior vegetation, without external ventilation, it could not permanently contain more than 365 persons without an increasing aerial contamination. But the vegetation in the large Crystal Palace, this island of ours, is being constantly reduced in amount. Enormous tracts of land once cultivated have been appropriated by highways and railways, and works and habitations, and at the same time there has been an enormous increase in the output of carbonic acid and the demand for oxygen by combustion in the consumption of fuel by manufactures of all kinds and for domestic purposes and by the respiration of animals and human beings. The revivification of the air by home industry is gradually decreasing, and the day may come when we shall be entirely dependent on imported oxygen as well as imported food, and will have to trust to the ocean to dispose of our surplus carbonic acid. At present the air of our large towns and especially those with narrow streets and towering buildings is often a very deleterious compound.

It is to the rise of the suburb—the island suburb—set in a sea of chlorophyll easily accessible, well planned, honestly built, that we must look in the first instance for the removal of some of the afflictions that overcrowding has brought upon us. But the suburbs, while it may do much, can not do everything, and there are other sources of relief, which it is our duty to turn to and to improve. We must take measures to reduce the influx of population into our already congested towns, and to keep on the land those who have been born and brought up on it and to bring back to the land those who have inconsiderately left it. Beyond the city and its satellites, we must afford to those who are weary of the dirt, confinement, dreariness and ugliness of over-crowded quarters, room and opportunity for healthy, moral and physical life. And there

are several ways in which this can be done which I can but name. We can create new cities on new sites, with all the advantages and none of the drawbacks of the old ones—garden cities of the type so eloquently and convincingly advocated by Mr. Howard, in which the needs of industry and the needs of humanity will be reconciled. Charles Kingsley in his philanthropic ardor foresaw something of the kind for he dreamt of cities—which should be "a complete interpenetration of city and country, a complete fusion of their different modes of life and a combination of the advantages of both, such as no country in the world has ever seen." And his vision has come to pass. We have Bourneville and Port Sunlight—cheering oases in the industrial desert—and better still we have Letchworth, gradually coming into being, on a broader basis and with greater amplitude of design. Letchworth is still incomplete, but two visits to it have enabled me to appreciate the judicious way in which it has been mapped out, the excellence of all its sanitary arrangements, and the rapid progress it is making. It is full of promise, and it would, it seems to me, be a national calamity should any want of financial support prevent the project in its entirety from being carried to a successful issue. It is to provide for 30,000 inhabitants, and that will not be much of a depletion for congested London, but whenever Letchworth is an accomplished fact, other garden cities will be undertaken. The transference of manufacturing industries to the country is feasible; it has indeed been going on for some time both in this country and America, in the avoidance of high rents and rates, and where suitable sites in the country can be provided with suitable accommodation for workers, with cooperating industries around, and with facilities for obtaining power, industries will congregate and garden cities arise.

Another way in which we can tap our great cities of their clogging superfluities of population, is by establishing in our dominions beyond the sea, land colonies, under some such scheme as that so ably excogitated by Mr. Rider Haggard. There are in our cities crowds of men and women brought up on the land, who have drifted into the city, and tossed about there as social flotsam, miserable failures, who with families of young children would gratefully embrace the chance of returning to conditions such as formed the surroundings of their youth, and of rectifying their own mistakes, by placing their children's feet on the path of prosperity. Such families carefully selected, settled in parties, if possible made up from the same towns at home, in well chosen localities, under skilled and sympathetic management and with necessary financial assistance to start with would undoubtedly do well as have done the indigent settlers at Port Amity, while their removal would clear the air of our towns at home.

But the best of all methods and the most promptly available for

checking overcrowding in towns is by improving housing in the country. We are told that the flower of the agricultural class flock to the towns because they dislike the monotony of country life and long for excitement and variety. That is so, no doubt, to a large extent, and perhaps our present system of education is calculated to foster discontent with the peasant's lot, and engender vague ambition, restlessness, and a thirst for rousing stimulants, but I suspect that another contingent of country folk find their way into the towns, not so much attracted by their glamor as repelled by the dingy wretchedness they leave behind them. Agriculture is, after all, the most varied and least monotonous of employments, and could the cottages of the laborers be made wholesome and attractive, and the village life invested with some interest, many who now migrate to the towns would stay at home, and many who are in the towns and have tried them and failed, would be glad to be translated back to the land. I feel sure that many of the laboring class, under the goad of poverty, quit with reluctance the fields they have tilled and cast many a lingering longing look behind at the dilapidated hut that sheltered their childhood. The Anglo-Saxon has always had a deep-rooted attachment to his 'ham' and his 'ton.' Home sickness is not, perhaps, as common as it used to be, but the homing instinct still exists amongst us and is to be diligently kept alive if our race is to avoid disintegration. Nothing has seemed to me to presage the pouring forth of the vial of the seventh angel more imminently than the proposal, seriously made, that all infants at birth should be taken away from their parents and brought up in public institutions, foundlings of the state, a system under which all virtue would forthwith go out of us leaving us mere dry husks of mankind. And nothing has seemed to me more minatory for the future of our empire than the decay—for there is some decay—of home life amongst us. The affluent classes in great numbers voluntarily resign its charms for the luxurious indulgence of the club and restaurant, and the round of pleasure, as it is called; and the poor, through no fault of their own, are in large numbers deprived of them, for it is impossible for the tender associations of the home to evolve in a temporary burrow in a town warren. They have a tenement or lodging, but no home; but a vestige of the old home sentiment is sometimes seen, as Mr. Bray assures us, in the fidelity with which they cling in poverty to some bit of furniture—a table, a chair or clock that has stood the wear and tear of time. But if we are to rescue the submerged tenth and redeem the very poor, we must somehow see that decent homes are provided for them. The finest feelings, the firmest principles are nourished in the home; a genuine man's joys, hopes and ambitions should center in it. Truly did the great poet of my country—who was not only a poet but a deep-seeing social reformer—exclaim:

To make a happy fireside clime,
For weans and wife;
That's the true pathos and sublime
Of human life.

For the want of the 'happy fireside clime' the poor are doubtless themselves often to blame. Of the idleness, thriftlessness and drunkenness that keep them poor and homeless, I need say nothing; these are the theme of daily homilies, but there is a habit they have formed which I think in some degree contributes to their penury and is worth noting, and that is the habit of wandering purposely from place to place—a phase of the *mania errabunda*—as it has been called, which possessed the 'Ancient Mariner' and keeps the tramp and the globe-trotter moving on. Without any valid reason, large numbers of them are constantly changing their abodes, and an enormous sum is spent annually on removals that might be more profitably employed in making the home habitable. Removals are, of course, largely instigated and justified by the search for work or for better surroundings, by growing family requirements and improving circumstances, but beyond all that, they go on, on the large scale, simply to gratify the love of change or in a foolish spirit of rivalry. These poor people keep shifting about in sheer restlessness; having dirtied or damaged one dwelling they pass on to another. A friend of mine in Scotland built some model cottages for his laborers, and on visiting them was surprised to find that the bedrooms upstairs were unoccupied and had been converted into stores for apples, onions and potatoes, while the families were herded together below. On inquiring the reason of this, he was told that these laborers didn't care to have more furniture than they could conveniently move in one cart. This sort of thing is very inimical to home making, for the home is a slow growth, that does not, like Jack's bean stalk, shoot up in one night, but must have time to take root and won't bear frequent transplanting. And it is inimical also to success in life. Mr. Patterson, the master mechanic of the Grand Trunk Railway, says: "I find among the class of workmen that comes from the Old Country, there is a great tendency to run from one situation to another; in fact a number of them seem to have an aversion to permanent employment. This wandering spirit is very detrimental to a man's progress."

But apart from the improvidence and stupidity of the poor themselves, or anything like it, the home is still beyond the reach of many of them and in some districts it is a vanishing quantity. It is for the sanitary and the social reformer to work together to resuscitate the home, to augment the taste for it and make it more and more palatable in town and country.

On some future occasion you will, perhaps, allow me to say something about the town-homes of the poor and to offer a few suggestions

in connection with the highly complicated problems they present for our consideration, amongst which suggestions, the not least prominent will be one for the strengthening of your hands. I feel keenly, that if the housing question in towns is to be adequately dealt with the sanitary inspector must have more power in his elbow than he has hitherto had. He must have security of tenure, and I am glad to be able to tell you that the preventive medicine section, over which I presided, at the recent Public Health Congress in London, passed a resolution desiring the council to represent to the government the urgent importance of giving security of tenure to the sanitary inspector, as well as to the medical officer of health. Then, the sanitary inspector must have more effective control over the nuisances he discovers, and the only way to give him that is to make his 'intimations' equivalent to a legal notice. These intimations are, I understand, now often treated as waste paper. There are agents and owners of property, of the baser sort, who delight in thwarting and putting obstacles in the way of sanitary inspectors, and to such gentlemen I should give short shrift, showing no particular indulgence to the slum-owner generally. Dr. Harris, medical officer of Health for Islington, reported lately, that in his district 60,296 visits were last year paid by the sanitary inspectors to 7,133 properties, on an average 8½ visits to each. That indicates, I think, much passive resistance, much waste of energy, much unnecessary maintenance of dangers to health, and I agree with Dr. Harris that in this matter 'Law ought to be brought into line with common sense' without delay.

It is to rural housing, more especially in its relation with the relief of overcrowding in towns, that I had intended to direct your attention to-day, but my excursions into the approaches to that subject have left me only a few minutes in which to touch on it. The main point, however, is—and on that I have already insisted—that by improving our country cottages and adding to them cottages of an approved type, we shall in some degree check the exodus from the country and even set up a back-wash from the towns. And in order that we may do that we must have amended the building by-laws that have been in no small measure answerable for the depopulation of rural districts and for the congested state of towns. That these by-laws require to be overhauled and remodeled, no one who has read Mr. Wilfred Blunt's article in the *Nineteenth Century*, or the speeches made by the members of the deputation that waited on Mr. Walter Long, then president of the Local Government Board in November last, can doubt. The unfortunate clause in the public health act of 1875, providing that poor law districts might declare themselves urban districts and so require powers similar to those exercised in towns, and frame by-laws of their own, has been the source of all the mischief. Under this clause half the rural districts of England have acquired urban powers which, being

exercised by persons having for the most part an interest in urbanizing the district—jobbers in residential land ripe for development, tradesmen, contractors and local builders—have been used as an instrument to prevent the erection of dwellings suitable for agricultural laborers and to tie the hands of landowners willing to provide such dwellings. No better example of this can be adduced than Mr. Wilfred Blunt's own case. Having himself experimented with an iron bungalow which he found singularly comfortable and commodious, he directed his estate carpenter to erect on his property in the New Forest where there are no builders' by-laws, two cottages, intending should they prove successful to make them the model for cottage building in Sussex. And highly successful they proved. He found they could be erected at the cost of £130 for a building covering 700 feet area, with a verandah of 240 feet more and an outbuilding containing washhouse and closets—"as snug and sanitary a home as any poor man could wish to inhabit, for there was a fireplace in every room, roof ventilation and ample door and window space."

But when Mr. Wilfred Blunt came to Sussex, where the London building by-laws are in force, there was a lion, and a very fierce lion, in the path. The plan of a cottage was submitted to the rural council and no objection was taken to it until the building materials had been deposited on the ground. Then, however, notice was given that the plan was disapproved by the council as violating the by-law. This notice, Mr. Wilfred Blunt thought it his duty to disregard, and went on with the cottage, which cost £130, and which, with an additional quarter of an acre of land, he could let without loss at 2s. 6d. a week or 1s. a week less than an old cottage it replaced. But alas for rural economy! The builder was summoned for building with other than bricks and mortar, and an action was brought against Mr. Wilfred Blunt, as a result of which a continuing penalty of two shillings a day was inflicted on him until the model cottage was pulled down.

It is clear that a check must be administered to rustic Bumbledom, and a stop put to the application to purely agricultural areas of regulations intended for towns, and which in towns have had an altogether salutary effect in preventing the construction of unsafe and insanitary houses. But I can not go as far as some who have urged that there should be no by-laws in country districts, or that such by-laws should not apply to any new building on a freehold property where such building is more than a given number of yards from other dwellings or past the property of adjacent owners. In regard to sanitary arrangements, by-laws seem to us as necessary in the country as in the town. It is not licence, but reasonable liberty that is wanted; not looseness, but elasticity, and it is to be hoped that this will be realized in the model code of by-laws for rural districts promised by Mr. Walter Long, and

in which cottages in certain situations are to be permitted of wood or other material.

A powerful impulse has been given to improvement in country housing by the articles on the subject which have appeared in the *Country Gentleman* and *Spectator,* which have, as by natural magic, invoked the enchanting village at Letchworth, which is now on exhibition, and which I would recommend every sanitary inspector concerned in rural affairs to visit and study diligently. He will there, while enjoying a pleasant picnic, have an instructive lesson, and be able to satisfy himself that a serviceable and comely cottage, in all respects suitable for a laborer or working man and his family, can be built for £150, including builder's profit. He will there see cottages of many different patterns, and built of many different materials, of stone, wood, brick, iron, concrete, cement, steel and plaster in various combinations, and will obtain from the catalogue full information about the price and specifications of each. He will see a marvelous display of ingenuity and contrivance in the fitting in, of domestic requirements and of making the most of next to nothing. No doubt his critical eye will detect flaws here and there, but everywhere he will perceive an intelligent deference to the claims of modern sanitation. The cottages vary greatly; each has an individuality of its own, but sunniness, airiness and coziness characterize almost all of them. They appeal to many tastes, but to no tastes that are vulgar or debased. They are pretty, but their prettiness is subordinate to their utility; they are picturesque, but not pretentious. Simplicity and cleanliness are the dominant ideas, and they are cheap with a cheapness that is unbelievable until they have been actually seen and examined, and compared with the estimates. Think of a detached cottage, well proportioned and artistic in design, with a living room with range 15 ft. 6 in. x 11 ft. 4 in., scullery with bath, hot and cold water, 9 ft. 4 in. x 7 ft. 6 in., three bedrooms 9 ft. high, 13 ft. 4 in. x 9 ft., 13 ft. 4 in. x 9 ft. and 8 ft. 6 in.; with pantry, two cupboards, coal hole, shed for wood, water closet, water laid on, drains connected, rain water butt, floor of scullery and pantry tiled, and say whether it is dear at £150.

There are cottages at Letchworth adapted to different climates, and suitable to different districts according to the different material of which they are constructed. I saw some that I thought would scarcely survive a blast of Boreas on a Scotch hillside, others that could withstand a hurricane; some that would be in place where timber is abundant, others where brick or iron are in the ascendant.

The village at Letchworth, unique in its diversity of cottage contours, planted on a bare common, but in sight of stately elm trees circling a venerable church, and gaudy with many floral dyes and green embroideries, recalls, of course, Mrs. Heman's verse:

The cottage homes of England!
By thousands on her plains,
They are smiling o'er the silv'ry brooks,
And round the hamlet fanes.
Through glowing orchards forth they peep,
Each from its nook of leaves,
And fearless there the lowly sleep
As the bird beneath their eaves.

They recall that verse, but with a difference, for while Mrs. Heman's cottage homes gave delight to the eye by their rugged and peaceful external beauty, they must have brought anxiety to the soul of the sanitarian who peeped into them by their primitive squalidity. The cottage homes of Letchworth are not less gratifying in their sanitary than in their esthetical aspects, and may be slept in by their lowly inhabitants with a sense of security that the cottagers of a century ago, when typhus and smallpox patrolled the land, where scarcely entitled to feel. These cottages have dealt the death blow to foolish restrictions on country housing . That village leads the way in a new movement to whicl. all sound sanitarians will cordially wish success.

A survey of some of these cottages at Letchworth, so quaintly pretty, so minutely commodious, so hygienically correct, so reasonable in price, suggests that they should have attractions for the well-to-do, not less than for the laboring class. Perched on some beetling cliff or breezy down, bosomed in some bosky dell, or planted in the fields neighboring some quiet hamlet, they would form a delightful week-end or holiday resort for families of moderate means. For children living in London, or other populous city, the seaside town, with the vulgar riot of the sands, is not the place in which their vacations can be most healthfully or profitably spent. They should be brought into living contact with nature, be enabled to form friendships with trees and animals, to pry into the secrets of insects and birds, and taught to take more pleasure in the hedge rows with their 'profuse wealth of unmarketable beauty,' than in the shop windows with their flaunting temptations. The cheap cottage as a holiday-home would create new family affinities, promote the unfolding of faculties that are apt to remain dormant or stunted amongst bricks and mortar, and teach self-help and independence instead of the feeble snobbery that meets every want as it arises by ringing the bell, for I saw no bells, electric or other, in these cottages. It would obviate the apprehensions of infection that are often not unjustifiably felt in taking possession of lodgings or furnished houses at seaside resorts, and elicit taste and ingenuity as no mere hired and temporary residence can do. Its decorative improvement would be a recreation, and the owner would be proud to say of it—'a cheap thing, but mine own.' It is thought sometimes that

frequent changes of scene are wholesome and educational for children, and so they are shifted year after year to one watering place after another, which, in so far as they come within their observation, are all very much alike. No doubt a variety of new impressions and world-wonder are valuable, but these should come after childhood is over. The apprenticeship should precede the travels, and for young children it is far better that they should be allowed to wind their affections round some one spot of earth, and slowly to form tender associations which will be sustaining and gladdening to them to the end of their days, than that they should dissipate their interest in sight-seeing, and be pleased with a succession of images as immemorable and unemotional as the figures of the kaleidoscope. The farm house has no doubt many advantages as a holiday retreat, but the cheap cottage, as a family seat and permanent possession, is infinitely superior. I hope that some of my sanitary inspector friends in the large towns may see their way to acquire one in some suitable locality. Of well-selected plan, and with some small attractions and additions such as my sanitary inspector friends will well know how to devise, raising the price somewhat upon that of the Letchworth model, but still leaving it within the category of cheapness, such a dwelling should be a source of health and pleasure, and also a good investment.

I have taken a wide, a hurried, and, I fear, a somewhat confusing sanitary outlook, but, when I meet you, so many topics in which we are mutually interested press for attention that I am tempted to attempt too much. If at any point in the outlook I have interested you, or suggested to you some new thought, or some new aspect of an old thought, I shall be well content.

SHORTER ARTICLES.

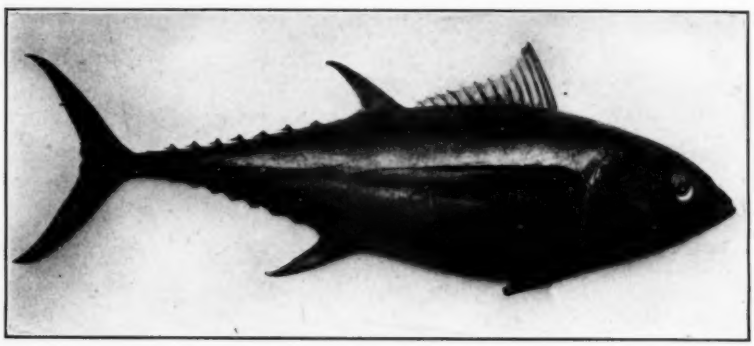
THE YELLOW-FIN ALBACORE IN CALIFORNIA.

DR. CHARLES FREDERICK HOLDER, of Pasadena, reports that the coast of California has recently been invaded by a splendid new game fish, the Japanese hirenaga or kihada maguro called yellow-fin albacore, *Germo macropterus* (Schlegel). This has appeared in considerable schools off Santa Catalina Island in southern California. I enclose a photograph of a specimen taken near Avalon, sent me by Dr. Holder.

The yellow-fin albacore is common in southern Japan, being often seen in the markets of Nagasaki, where it is conspicuous from the bright yellow color of its dorsal and anal finlets. It is occasionally taken about Hawaii. It is described in the Hawaiian report of Jordan and Evermann as *Germo germo*, with a figure of *Germo germo* (*sibi*) copied from Schlegel's 'Fauna Japonica.' The yellow-fin albacore is, however, distinct from *Germo germo*, and its proper name is *Germo macropterus*, or *Thunnus macropterus*, if we place the long-finned albacores in the same genus as the sunny or tuna (*Thunnus thynnus*).

From the related species with long, ribbon-like pectorals, *Germo macropterus* is known by the yellow finlets and by the very high, falcate, soft dorsal and anal fins.

Dr. Holder writes that tuna has almost deserted Santa Catalina for the last three years, owing perhaps to the presence of a school of large and very hungry killers (*Orca*), 'but the yellow-fin albacore, coming so suddenly, gave almost as much sport to the anglers.' The weight of the fish is about fifty pounds.



YELLOW-FIN ALBACORE (*Germo Macropterus* Schlegel).

DAVID STARR JORDAN.

THE PROGRESS OF SCIENCE

SAMUEL PIERPONT LANGLEY

In the death of Dr. S. P. Langley, secretary of the Smithsonian Institution, America loses one of its most eminent men of science. Langley was born in Roxbury, Mass., on August 22, 1834, descended from long lines of New England ancestry. As a boy he was interested in astronomy, radiant energy and mechanical flight, and with his brother, now Professor John W. Langley of the Case School of Applied Science, he constructed telescopes. He did not enjoy or suffer a college education, but practised civil engineering and architecture, until, at the age of thirty-one, he became assistant in the Harvard Observatory. In 1866 he went to the U. S. Naval Academy as assistant professor of mathematics, and the following year was made director of the Allegheny Observatory and professor of astronomy and physics in the Western University of Pennsylvania. In 1887 he was appointed assistant secretary of the Smithsonian Institution and succeeded to the secretaryship on the death of Baird the same year. This was then, as it is perhaps still, the most responsible position and the highest honor that can be conferred in this country on a man of science.

Langley made his first published contribution to science at the age of thirty-five, it being a report on the total eclipse of August 7, 1869, observed in Kentucky. In the following year he again observed a solar eclipse, making observations on the coronal rays, and this was followed by his important researches on the solar photosphere. His work on the radiant energy of the sun resulted in and was promoted by the invention of the bolometer, an instrument which has been perfected to measure a millionth of a degree of temperature. This is accomplished by changes of electrical resistance due to heat and detected by a galvanometer, whose fluctuations may be photographed. Some of Langley's most important observations were obtained in 1881 on Mount Whitney at an elevation of 14,000 feet, and they have been continued to the present time in the Astrophysical Observatory, which was in 1891 established in connection with the Smithsonian Institution. Probably Langley's greatest work is connected with the heat of the sun and the infrared rays of the spectrum, but perhaps his researches on aerodynamics are equally well known. His theoretical and experimental contributions to this subject are of fundamental importance, in no wise lessened by the fact that he was unable to solve the practical problem of aerial flight.

Langley died from a stroke of paralysis on February 27. A sketch of his life by Dr. E. S. Holden with a portrait was published in Volume XXVII. of this journal. The frontispiece to the present issue shows Dr. Langley in the robes in which he received the D.C.L. degree from Oxford University. The regents of the Smithsonian Institution have passed the following memorial resolution:

Resolved. That the board of regents of the Smithsonian Institution express their profound sorrow at the death, on February 27, 1906, of Samuel Pierpont Langley, Secretary of the Institution since 1887, and tender to the relatives of Mr. Langley their sincere sympathy in their bereavement.

That in the death of Mr. Langley this Institution has lost a distinguished, efficient and faithful executive officer, under whose administration the international influence of the parent institution has been greatly increased, and by whose personal efforts two important branches of work have been added to its care—the National Zoological Park and the Astrophysical Observatory.

That the scientific world is indebted to Mr. Langley for the invention of important apparatus and instruments of precision, for numerous additions to knowledge, more especially for his epoch-making investigations in solar physics, and for his efforts in placing the important subject of aerial navigation upon a scientific basis.

That all who sought the truth and cultivated science, letters and the fine arts, have lost through his death a co-worker and a sympathizer.

That the executive committee be requested to arrange for a memorial meeting to be held in Washington.

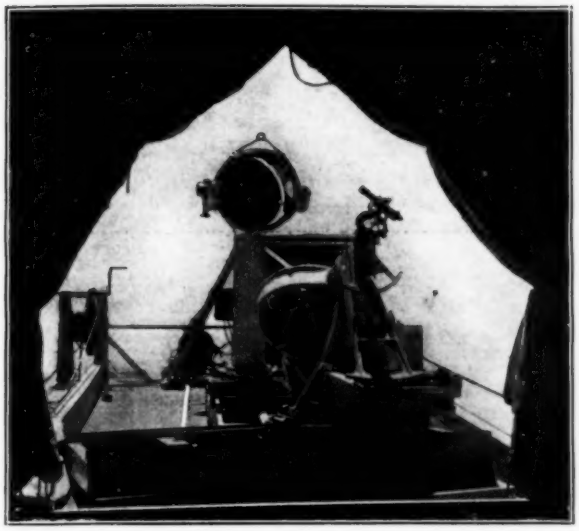
That Dr. Andrew D. White be invited to prepare a suitable memorial which shall form a part of the records of this board.

THE SOLAR OBSERVATORY OF THE CARNEGIE INSTITUTION

EARLY in its history the Carnegie Institution of Washington showed marked interest in the encouragement of astronomical research. In 1902 Secretary S. P. Langley, of the Smithsonian Institution, recommended the establishment of a lofty solar observatory, for the special purpose of measuring the amount of the solar radiation. In regard to the practical value of such studies Mr. Langley's own words may be quoted: "All the nebulæ in the sky might be blotted out without affecting the price of a laborer's dinner, or the material comfort of a human being. What shall we say of a similar contingency to the sun? While a slight variation in the radiation of the sun may conceivably cause the death of millions of men by famine, it certainly seems worth while to look at it from its utilitarian as well as from its purely scientific interest. It is the possible immense *utility* of the solar observatory that I dwell upon, and concerning which I may borrow the weighty words of Professor Newcomb in a similar connection, and state that astronomical research may bring to light not merely interesting cosmical processes but 'cosmical processes pregnant with the destiny of our race.'" And in another place he says: "Though we may never hope to affect the original source of solar radiation by any human effort, there is every hope that we may learn to forecast its effects upon the earth and provide for them."

A special committee was appointed to investigate the plan, and, as a result of extended investigations, Mount Wilson, in California, was chosen for a

THE COURT OF THE MONASTERY.



CŒLOSTAT AND SECOND MIRROR OF NEW TELESCOPE.

provisional site, and $10,000 was appropriated for a thorough trial of the mountain. Later the institution decided to undertake the construction and equipment of a solar observatory at this point and up to the present time has devoted more than $300,000 for the purpose. Professor George E. Hale, former director of the Yerkes Observatory, has been made permanent director, and under his care the installation of the observatory has gone forward with characteristic energy. At the present time the Snow horizontal telescope and various buildings necessary for the proper execution of the work of such an observatory are nearing completion. The program of work which Professor Hale has laid out, in a very condensed form, is as follows:

The ends sought are two:

(1) The Study of the Sun as a Typical Star.

(2) The Study of the Sun as the Center of the Solar System.

The investigations include:

I. Direct photography.
   - (*a*) Daily photographs of the Sun on a Scale of 6.7 inches.
   - (*b*) Large scale photographs of spots and other regions.

II. Photographic Studies of the Solar Atmosphere with the Spectroheliograph.
   - (*a*) Daily photographs of the Sun taken with H_1, H_2, and Hδ, and other dark lines for calcium flocculi and prominences.
   - (*b*) Measurement and discussion of the photographs.

III. Spectroscopic Investigations.
   - (*a*) Daily photographs of spectra of spots.
   - (*b*) A study of velocity of motion of flocculi and prominences.
   - (*c*) Bolometric measurements of relative radiation of sun-spots, faculæ, and photosphere.
   - (*d*) Spectroscopic measurement of solar rotation.

IV. Studies of total solar radiation.

V. Comparative laboratory investigations.

The Snow horizontal telescope is provided with two concave mirrors, one of 60 feet focus, and the other of 145 feet focus, either of which can be used as desired. The beam of sunlight falls first upon a plane mirror of 30 inches diameter, whence it is reflected to a second plane mirror of 24 inches diameter, which sends it to the concave mirror of the telescope. With the image thus obtained any of the pieces of work outlined above may be carried out.

The spectroheliograph is the creation, in large part, of Professor Hale himself, in whose hands it has led to remarkable results. The principles involved seem to have been suggested first

by Janssen, but the first practical results were obtained by Hale. The principle of the instrument is simple, but the construction and manipulation of a spectroheliograph which will give the best results calls for the highest skill, while the interpretation of the results obtained taxes the keenest minds. With an ordinary slit spectroscope one may obtain the familiar solar spectrum. Crossing this luminous band are the relatively dark absorption lines characteristic of the various elements found in the solar atmosphere. Each line corresponds to light of a certain wave-length. If now this spectrum is all covered, with the exception of a single line, *e. g.*, the Hβ line of hydrogen, and a photographic plate is placed behind this screen and the spectroscope is moved at right angles to the optical axis, an image of the sun and prominences in monochromatic light is obtained on the plate from successive images of the slit.

With such an apparatus Professor Hale is planning to photograph the sun and prominences each day in various kinds of monochromatic light, one photograph showing a hydrogen sun, another a calcium sun, etc. Recently he has found it possible by setting the slit with special care to obtain photographs of calcium vapor of different densities, and hence at different levels.

Such studies must add greatly to our knowledge of the sun, especially since Mr. Abbot, Dr. Langley's able associate in bolometric determinations of the solar constant, has made a series of observations at Mount Wilson. The original plan for a solar observatory included an elaborate study of the solar constant and any changes in its value, extending over at least one sunspot period. It was also proposed to have one station at as great an elevation as possible in connection with a lower station, in order to measure the absorption by the earth's atmosphere, and so eliminate it from the determination of the solar constant. To this end an auxiliary station will probably be temporarily occupied on a mountain not far from Mount Wilson at an elevation of some 12,000 feet. Mount Wilson itself has an altitude of 5,886 feet, and overlooks the city of Pasadena, southern California.[1] With the completion of the solar observatory not only will the summit of the mountain make a picturesque sight, with its various instruments and buildings, but from this lofty spot may come much of interest, and perhaps of utility, for the human race.

THE WARREN MUSEUM OF NATURAL HISTORY

THE contents of the Warren Museum of Natural History have been acquired by the American Museum of Natural History, New York City. The collection was made by Dr. John C. Warren, who is little more than a name to the present generation, the son of John Warren, first professor of anatomy and surgery at the Harvard Medical School. He became adjunct professor to his father in 1809 and succeeded him in 1815, occupying the chair till 1847, when he retired at the age of sixty-nine. Besides being an eminent surgeon he was an enthusiastic anatomist, comparative as well as human. It is through him that Massachusetts has the honor of having passed the first anatomy law in America in 1831, one year before Great Britain. As a token of consistency he left orders that his body should be dissected and the skeleton kept in the Warren Museum of the Harvard Medical School (not to be confounded with the above-mentioned one). It hangs there to-day. In his later years he found more time to devote to science. In 1848 he was chosen president of the Boston Society of Natural History. In 1852 he published a very handsome monograph on the *Mastodon giganteus*, the finest specimen of which is the centerpiece of this collection.

[1] During the years 1889 and 1890 a temporary astronomical station was maintained at the summit by the Harvard College Observatory.

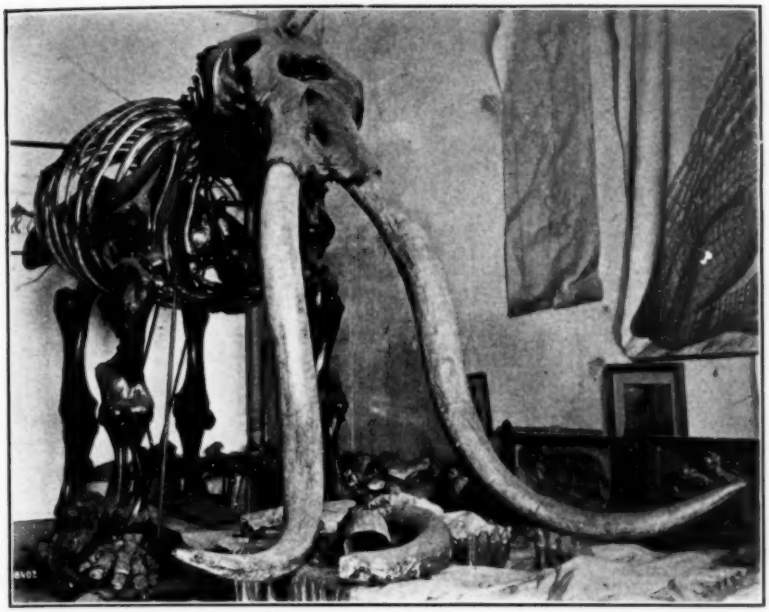


FIG. 1. RESTORATION OF THE WARREN MASTODON.

The history of the 'Warren Mastodon,' found near Newburgh, N. Y., is briefly as follows: In the very dry summer of 1845 diggers in a swamp, the bottom of which had never been seen above water, came upon the head two or three feet below the surface towards evening on August 12. The next day they unearthed the head and then, working backwards, the spine and ribs with the pelvis. While the head was being raised, one tusk parted near the middle and the other cracked at about a similar point, but did not then come to pieces. The feet of the hind legs were found near the shoulders (Fig. 2), showing clearly that the animal had been mired. On the second night the walls of the excavation caved in, covering and disturbing the unearthed parts. This is probably the reason that the end of the tail and many of the last phalanges are missing. Fig. 2 is a photograph of the vignette in Dr. Warren's monograph. Beside giving a view of the country it represents diagrammatically the geological formation. The tree is growing on the very edge of the section. Just below it is a dark layer of peat, followed by a shallower layer of red moss. Then comes one of shell marl in which reposed the more superficial part of the body and the right fore-limb. The rest lay in mud, apparently contained in a cavity formed by clay, which in the diagram descends obliquely on the reader's right. The skeleton was mounted after a fashion and exhibited about the country for some months, when it was purchased by Dr. Warren and mounted anew by Dr. N. B. Shurtleff. The great tusks were unable to support themselves and were replaced by imitation ones, the fragments being preserved. The small tusk of the lower jaw was preserved also, and it is hoped will be found in some safe place in the museum, but its whereabouts is at present unknown. Many, if not all of the terminal phalanges and perhaps a yard of the tail are false, as is also a part of the breastbone, which was broken, probably by the cave-in. Otherwise the skeleton is perfect and the finest in existence. The skeleton of the

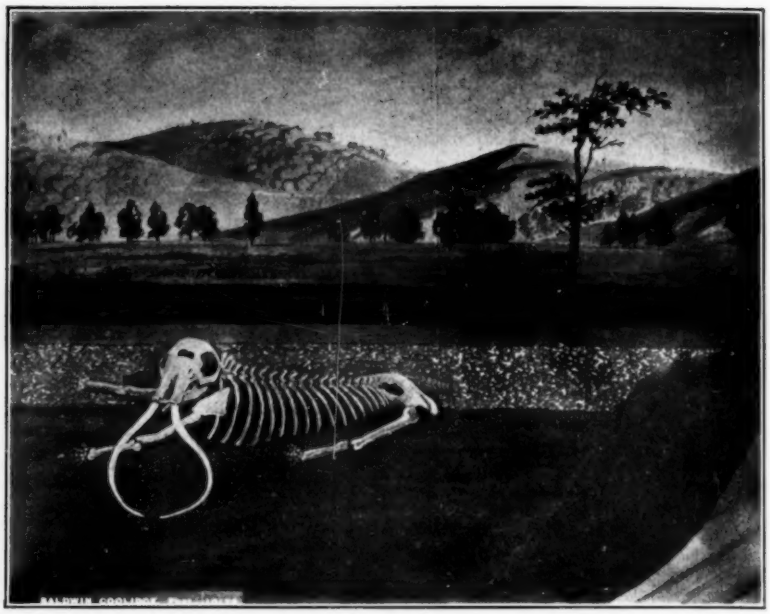


FIG. 2. IMAGINARY SECTION, showing the geological formation at Newburgh, N Y., in which the Warren Mastodon was found.

elephant 'Pizarro' seems insignificant as it stands beside the mastodon in the brick and iron building in which Dr. Warren placed his treasures of natural history. The collection is particularly rich in mastodon remains. There is the 'Shawangunk' head, which is even larger than that of the Warren mastodon. There are many disarticulated bones of the 'Baltimore' mastodon, as well as many mastodon and mammoth teeth. The vertebræ with fragments of the head of a great zeuglodon, a fossil whale are arranged round the room. A noteworthy feature is the collection of large and small slabs of remarkably fine fossil foot-prints from the Connecticut River valley. Space allows no mention of many casts of fossil remains, still less of mere curios.

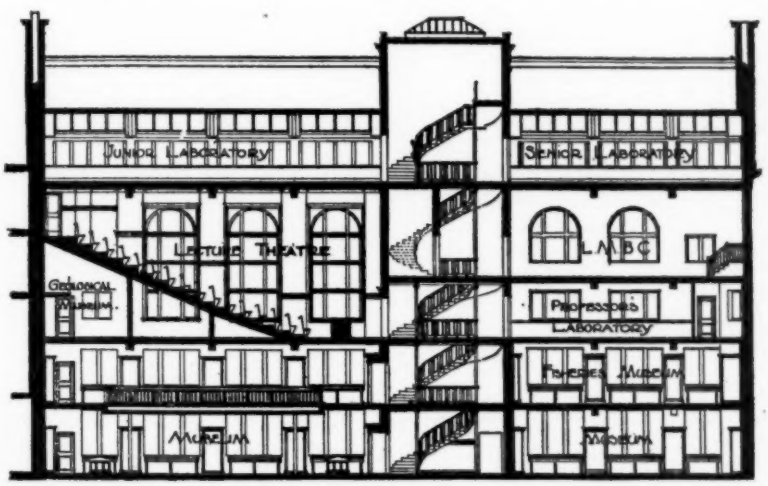
THE NEW ZOOLOGICAL MUSEUM AND LABORATORIES OF THE UNIVERSITY OF LIVERPOOL

THE most significant movement of higher education in England is the development of the newer universities. While Oxford and Cambridge have voted to admit no students without an examination in Greek, the University of London and the provincial universities are paying increased attention to scientific education and research. All these institutions are of late foundation, and they have only recently assumed the name and functions of universities. University College, Liverpool, opened in 1882, has only within the past two or three years assumed the name of the University of Liverpool. A few words in regard to the new zoological laboratories, opened on November 18, will call attention to the progress of the university.

The accompanying illustration shows the new building for zoology, with the adjoining Hartley Botanical Laboratory and the building for electrical engineering. A cross-section of the building for the zoological laboratories is also given. It has a frontage of 123 feet, is 41 feet in depth and 84 feet in

THE ZOOLOGICAL LABORATORIES OF THE UNIVERSITY OF LIVERPOOL.

height from the street level, and is built of red pressed brick relieved with white sandstone. The building consists of a central tower containing the entrance hall and staircase and some of the smaller rooms on each floor, and of two blocks, the north and the south, which have been treated rather differently as regards internal structure. The south block has only three main floors while the north has five in the same height. The central tower extends a story higher. In the south block the three floors accommodate (1) the museum with its large gallery, (2) the lecture theater and (3) the large junior laboratory at the top of the building. In the north block, on the



CROSS-SECTION OF THE BUILDING.

two lower floors there are extensions of the museum to receive special collections, and the rest of the space is devoted to the senior class-room, senior and honors students' laboratories, the departmental library, and rather large laboratory and storeroom accommodation for the sea-fisheries department, the work of the economic entomologist, of the marine biological committee and other practical applications of zoology.

The department of zoology is under the direction of Dr. W. A. Herdman, who was appointed to the Derby chair of natural history at the opening of the college. He is well known for his zoological researches, especially in marine biology. Readers of this journal will remember with interest two articles contributed by him—one on the Naples Zoological Station, the other on the pearl fisheries of Ceylon. Dr. Herdman has not only made the University of Liverpool a center for zoological research, but has developed in connection with it a marine biological laboratory on the Isle of Man.

SCIENTIFIC ITEMS

REAR ADMIRAL COLBY M. CHESTER, superintendent of the U. S. Naval Observatory, was placed on the retired list on February 28. He will be retained in temporary active duty in the Bureau of Navigation. Rear Admiral Chester will be succeeded in charge of the Naval Observatory by Rear Admiral Asa Walker.

PROFESSOR A. A. MICHELSON, of the University of Chicago, and Professor F. Kohlrausch, of Berlin, have been elected honorary fellows of the Physical Society of London.—Sir William Crookes has been elected a corresponding member of the physical section of the Paris Academy of Sciences in succession to the late M. Bichat.—Professor J. J. Thomson, of Cambridge; Mr. Oliver Heaviside, of London; M. Henri Becquerel, of Paris, and Professor P. Zeeman, of Amsterdam, were made honorary doctors of the University of Göttingen, on the occasion of the dedication of the new physical laboratory.

THE fiftieth anniversary of the connection of Professor Frederic Ward Putnam with Harvard University has been celebrated by the presentation of a volume, containing autograph greetings from forty of his former students. Dr. H. C. Bumpus, director of the American Museum of Natural History, has been authorized by President Jesup to offer Professor Putnam ethnological material sufficient to illustrate fully the life of the inhabitants of the Philippine Islands, leaving him to make such disposition of the collection as he may think best.—At a meeting held at the Mansion House, London, on February 27, to celebrate the fiftieth year of the foundation of the coal-tar color industry, and to take steps to do honor to Dr. W. H. Perkin, the founder, it was decided that an appeal be made for subscriptions for the purpose of carrying out the following objects: (1) The presentation to Dr. Perkin for his life time of an oil portrait of himself, the portrait to become the property of the nation at his death. (2) The execution of a marble bust of Dr. Perkin to be placed in the rooms of the Chemical Society. (3) The establishment of a 'Perkin Research Fund' for the promotion of chemical research to be administered through the Chemical Society.